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"Radical Subjectivism": Not Radical, Not Subjectivist **John O'Neill**

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CURRENT DEVELOPMENTS IN COST ACCOUNTING AND THE DYNAMICS OF ECONOMIC CALCULATION

THOMAS C. TAYLOR

The role of accounting as articulated in Austrian economic theory is securely fastened to the task of economic calculation. Ludwig von Mises distinguishes between prospective or anticipative calculation involving the "precalculation of expected costs and expected proceeds" relating to a planned course of action and retrospective calculation that establishes the results of past action, the "accounting of profit or loss" (1949, p. 229). And his devastating argument that the inability of the socialist planners to calculate the costs of alternative courses of action renders socialism impossible and establishes the critical role of cost accounting in business decisions under capitalism.

The history of cost accounting dates back to the 1800s with the emergence of large enterprises like textile mills, railroads, steel companies, and retail companies. The need for measuring efficiency and determining the cost of converting raw materials and transporting goods or passengers gave rise to management accounting systems to support the profit-seeking activities of entrepreneurs. By the late nineteenth century, cost standards for improved efficiency and pricing were developed. And by 1925, according to Johnson and Kaplan, virtually all of the traditional cost accounting[1] practices prevalent today had been developed, including "cost accounts for labor, material, and overhead; budgets for cash, income, and capital; flexible budgets, sales forecasts, standard costs, variance analysis, transfer prices, and divisional performance measures" (1987, p. 12).

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[1]The term "cost accounting" is often used interchangeably with the term "management accounting." However, the latter term is a broader term whose meaning typically includes "cost accounting" as well as other topics such as budgets, sales analysis, and return on investment measures.

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One of the long-standing difficulties in cost accounting has been the allocation of "indirect costs" to various objects such as products, departments, divisions, etc. For example, traditional cost accounting has long depended on predetermined overhead rates in order to apply the indirect cost of manufacturing to different products by means of some base such as direct labor cost or direct labor hours. Mises was well aware of the problem of assigning indirect costs for purposes of departmental profitability analysis:

> The elaborate methods of modern bookkeeping, accountancy, and business statistics provide the enterpriser with a faithful image of all his operations. He is in a position to learn how successful or unsuccessful every one of his transactions was. With the aid of these statements he can check the activities of all departments of his concern no matter how large it may be. *There is, to be sure, some amount of discretion in determining the distribution of overhead costs.* But apart from this, the figures provide a faithful reflection of all that is going on in every branch or department. (Mises 1962, p. 32; italics added)

During the past decade cost accounting has come under vigorous attack on the grounds that traditional approaches to allocating costs are fraught with considerable arbitrariness and contain substantial errors which can lead to misguided decisions dealing with such matters as pricing, outsourcing, capacity planning, and profitability analysis for various product lines and other segments of business activity. For example, a costing program at a Whirlpool Corporation appliances plant revealed that a traditional overhead allocation per unit of a particular product was $65.46 when a more accurate cost tracing should have assigned $728.92 (Greeson and Kocakulah 1997).

Such distortions stem from the fact that cost structures in many companies have undergone profound change. Escalating overhead and other *indirect* or "support" costs accompanied by a major diminution of direct labor cost as a proportion of total product cost have resulted from the increasing degree of skilled labor, automation, and computerization in manufacturing and other areas of business activity. In a typical plant today you will often find extensive sophisticated equipment with virtually no "direct labor" workers, i.e., workers who literally touch and handle each unit of product. Workers, now described as "knowledge workers," are busy overseeing computerized machines and "interfacing" with the production process through computer terminals located throughout the plant. At the same time other support activities beyond the factory, such as quality control, supply chain management, information technology, customer service, engineering, and new product development, have also risen significantly and become more complex, further increasing the level of *indirect* costs. As a result there are fewer variable direct costs and

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increasing pools of fixed indirect costs. This shift necessitates a far greater level of indirect-cost assignment and raises the potential for significant error in cost assignment under methods of traditional cost accounting which fail to reflect these cost structure changes.

The problem is exacerbated by the increase in cost objects.[2] Manufacturing technology now enables companies to produce customized products more readily, resulting in an increasing proliferation of products and services with fast-shrinking life cycles. In 1999 Michelin started selling some color-streaked tires and, commencing in the following May, shoppers could log onto the company's website and order *customized* color tires. This capability has expanded both volume diversity and product dversity across a wide range of products and services. As will be explained in more depth, product costs differ materially depending on volume or lot sizes and upon the degree of complexity associated with the production of different products. Also products and services are being delivered through multiple and, often, technologically new channels of distribution, like the internet, to different market and customer segments. To achieve competitive advantage, companies now place greater emphasis on determining not only *product* profitability but also profits relating to different *channels*, *territories*, and *customers*. This expansion of profit calculations across many product lines and other operational segments further elevates the impact of cost allocation.

The growing concern over cost assignment is related to the new era of dramatic global expansion of markets and phenomenal advances in information technology. With competition among business enterprises greater and more intense, customers continually raising their expectations, businesses devoting meticulous attention to the quality of products and services they provide and to developing strategies for competitive advantage, and ever-thinner profit margins, costing accuracy becomes more critical. While Mises apparently viewed the discretionary assignment of overhead as more or less innocuous, the drastic change in cost structures and the myriad of cost objects present a quite different picture today.

Does the current indictment of traditional costing suggest that economic calculation, so central to Austrian economic analysis and manifested in the cost accounting practiced in the day-to-day world of business, harbors at least glimpses of "just a system of groping in the dark" (Mises 1949, p. 700) that the socialist is doomed to use? Can we continue to refer blithely to economic

[2]The term "cost object" refers to any facet of the business which decisionmakers are interested in costing, including products, departments, divisions, activities, distribution channels, customers, and territories.

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calculation as "the compass of man embarking upon production" (p. 229) amid the *substantial* costing errors and inadequacies being discovered today in actual companies? These questions are not answered by the fact long stressed in Austrian analysis that entrepreneurs can and often do make errors in their judgments concerning the future, leading to lower profits or even losses. The latter errors are bound to occur due to the uncertainty everyone faces at every moment. Neither can they be answered by citing the calculation errors arising from monetary inflation which Mises has also addressed. The costing errors we are faced with here lie not in the uncertainty of the future or in the effects of money supply expansion but in approaches to costing which have been shown to be invalid, irrelevant, and inadequate under changing market conditions.

The answer to these challenges rests in the fact that economic calculation emerges in an evolutionary fashion by which traditional concepts and methods of costing can undergo dramatic change as a result of market competition—that while the *essence, idea,* or *concept* of economic calculation remains inviolate, approaches to its application are subject to refinement, improvement, and even fundamental alteration. The process of change emanating from market competition leaves no stone unturned, and even the very approaches to determining the monetary effects of market activity, either anticipated or actual, are not permanent. Thus economic calculation, indispensable to market relations, is no different from other resources of business activity in being subject to assessment and change to assure proper performance of its role in contributing to enterprise profitability. Shifts in the form or method of economic calculation in no way vitiates its critical role—in fact, as will be argued, it but confirms the absolutely indispensable role it plays. The essentiality of cost determinations to economic calculation, arguably the central pillar of Austrian economics, makes this issue of paramount concern.

The remainder of this article will discuss more fully the problem of traditional cost allocation as widely practiced today, and explain how some leading companies are implementing fundamentally new approaches to cost assignment and how these approaches create more effective tools for entrepreneurial decisions.

THE PROBLEM OF TRADITIONAL COST ALLOCATION

The assignment of *indirect* costs to products, departments, and other cost objects has been a long-standing problem in cost accounting. *Indirect* costs are distinguished from *direct* costs in the fact that *indirect* costs require some intervening factor or basis for being assigned to cost objects. Assigning the cost of manufacturing to various products offers a typical situation. Items like raw or direct materials and direct labor are directly traceable to different products,

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and, consequently, their costs are referred to as *direct* costs. On the other hand, the depreciation cost of the equipment used to produce the different products can be assigned to the products only through some method of allocation using a charge per unit of some intervening allocation base. For almost a century now traditional costing has resorted to predetermined "overhead rates" whereby such *indirect* costs are first related to a specific factor like direct labor hours or direct labor dollars.[3] To illustrate, if for a given period of operations the anticipated total overhead cost of items such as electricity, machine setups and repairs, depreciation, supervision, and insurance is $400,000 and the total direct labor hours is expected to be 100,000, an overhead rate of $4 per direct labor hour would be predetermined. The rate would then be used to assign the indirect cost of production to the different products according to the amount of direct labor hours charged to the different products. A given product or batch of product that consumed 800 hours of direct labor would be charged $3,200 of overhead in addition to its direct materials and direct labor costs.

The attack on this traditional approach to cost assignment rests on the valid argument that in two critical respects it is totally out of touch with reality in many companies today. First, direct labor is typically an insignificant factor in the total cost of a product, often less than 10 percent of the total. Second, direct labor is not a *causal* cost driver for most, if not all, manufacturing overhead. For example, machine setups and product inspections are not correlated with direct labor hours. With the mounting cost of overhead, stemming largely from increasing automation, indirect fixed labor, and computerization, along with the insignificant level of direct labor cost, the traditional method creates absurd overhead rates, like 1,000 percent of direct labor cost. As a result, managers are motivated to focus on reducing direct labor cost where small savings can have a huge impact on cost allocations and product costs rather than focusing on cutting overhead costs, which are increasing rapidly! (Could this explain some offshore outsourcing for labor-intensive processes, thereby mistakenly biasing the make-or-buy decision in favor of buying? Incidentally, offshore buying is not free of its own overhead cost elements relating to shipping, inventory tracking and management, etc.)

In their 1987 pioneering book, *Relevance Lost: The Rise and Fall of Management Accounting*, Johnson and Kaplan contend that by the 1980s cost accounting had abandoned, for some sixty years, the focus of early cost accounting (up to around 1925) on providing management with information relevant to strategic decisionmaking. In pursuing the question as to why traditional cost accounting has persisted in using inappropriate and inadequate approaches to

[3]The use of labor-based allocations of overhead has been widespread for decades.

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cost allocation for so long, they speculate that more accurate and strategically oriented costing failed to emerge due to the confluence of several factors: (a) the prohibitive cost of tracing costs more accurately in the early years; (b) the fact that early on direct labor was a substantial part of total manufacturing product cost and assigning overhead costs based on direct labor provided acceptable accuracy in product costing – meanwhile overhead costs were less prominent before the surge in automation and computerization over the past two decades; (c) from the early 1900s the dominance of financial accounting and the public auditing profession in reporting to outsiders – virtually equating cost accounting to determining the cost of inventory and cost of goods sold for disclosure in balance sheets and income statements; and (d) the acquiescence on the part of academia in defining the purpose of cost accounting "in terms of valuing cost of goods sold and inventories for financial reports, not for managerial decisions and control" (Johnson and Kaplan 1987, p. 135). This acquiescence has been manifested in both simplistic textbooks and other published writings – despite insightful works still pertinent today by a few academicians, such as J.M. Clark in 1920s, Ronald Coase during the 1930s, and William Vatter in the 1940s, which unfortunately went unheeded at the time. And so the question is raised: "What have management accounting practitioners been doing for the past sixty years?" Johnson and Kaplan sadly answer their own question: between 1925 and 1980, "virtually no new ideas have affected the design and use of cost management systems" (p. 176).

The dominance of the financial accounting mentality, strengthened by "generally accepted accounting principles" (GAAP) emerging from the Financial Accounting Standards Board (FASB) and mandates emanating from the Securities Exchange Commission (SEC) and the Internal Revenue Service (IRS), is also reflected in the usual treatment of costs incurred outside the factory. While manufacturing costs may be significant, there are many costs that relate to other facets of the business, including research and design, marketing, distribution, and customer service. Based on traditional financial accounting practice, these costs are aggregated and written off to the period as expenses; that is, as *period*, not *product*, costs. The errors arising from arbitrary overhead allocations to products are compounded by this failure to attribute to products the costs associated with nonmanufacturing resource consumption. The whole area of marketing and distribution costs has been glaringly neglected in cost accounting. It can be argued that the mandatory treatment of these costs, for financial reporting as well as tax purposes, as period costs and not product costs can be held responsible for this neglect.

In the task of evaluating the performance of different profit centers such as product lines, territories, and departments, the arbitrariness of traditional cost

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allocations is no less present than in the case of manufacturing overhead. To illustrate, the author recently participated in a costing project at a large hosiery division of a multinational corporation. The resulting study revealed that the current costing system was assigning the cost of new product development, an expensive engineering activity, to the different product lines based on dozens of product units sold. The preponderance of the engineering design activity actually could be traced to the complex, fashion lines sold in much lower volumes than the standard simpler styles. This meant that the complex styles were being grossly undercosted while the simpler lines were being overcosted using this costing approach. Distorted product-line profitability determinations were the inevitable result. The arbitrary assignment of indirect expenses based on relative sales is widespread in segment profit reporting. Of course, profitability distortions can lead to wrong decisions regarding the continuation, expansion, or discontinuation of different segments of the enterprise.

Traditional cost accounting then has been found obsolete and deficient for today's demands for greater cost accuracy. Arbitrary cost allocations do not recognize the actual causes of costs and thus fail to attribute costs to objects based on cause-and-effect. The seriousness of the problem has reached such proportions that in the past ten to fifteen years there has arisen a growing interest in another approach involving a radical departure from traditional cost allocation: *activity-based costing* (ABC). In the following section, ABC will be explained, followed by a discussion of how some leading companies are implementing ABC in order to achieve more strategically competitive decisions. Efforts to achieve more accurate cost information demonstrate the critical role of economic calculation and how market competition forces change in its methodology once its inadequacy calls for it.

ACTIVITY-BASED COSTING

Activity-based costing (ABC) restates the relationship between costs and the objects to be assigned costs. Unlike traditional cost accounting, which sees cost objects like products and divisions as directly causing the consumption of resources, ABC holds that costs, except for materials costs, are caused by the *activities* that make up business operations. Cost objects necessitate the performance of certain activities. It is the activities that consume the resources. By determining the costs of different activities as a first stage in the costing process, objects can ultimately be assigned costs according to their demands for activities. Separate predetermined activity rates are thus used rather than the amorphous overhead rates of traditional cost allocation.

ABC is fundamentally different from traditional costing because it insists upon explicit *cause-and-effect* connections between the various activities and

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costs (or consumption of resources) as a first stage; assigning costs to objects follows in a second stage involving the linkage of objects to activities through the use of activity cost drivers. An activity cost driver is "a quantitative measure of the output of an activity" (Kaplan and Cooper 1998, p. 95). To illustrate, consider the activity of setting up a machine to process different batches of materials. The activity cost driver would be setups or setup hours. Assume that total setup cost is $44,800 for a specific period of production. If the projected total number of setups is 70, then an activity rate of $640 per setup would be used to cost different products depending on the number of setups required. Obviously, products manufactured in small lot sizes will be assigned disproportionately more of the setup cost than those produced in large runs. To illustrate further, assume that 1,000 units of Product A are produced each period requiring 30 setups to accommodate the small individual orders of customers. Also, Product B is a large volume item involving the production and sale of 20,000 units and only 40 setups each period. Applying the setup rate of $640 per setup, Product A would be assigned $19,200 and Product B $25,600 for a total setup cost assigned of $44,800.

The important point to be observed is the significant amount of setup cost assigned to Product A *in view of its relatively small total quantity (1,000) in comparison to the total quantity of Product B (20,000).* Product A involves less than 5 percent of total volume, yet it has been assigned almost 43 percent of the total setup cost. This disproportionate cost assignment is attributable to the fact that Product A averages only approximately 33 units for each production run or setup while Product B averages 500. This observation is important because traditional overhead allocation methods based on direct labor hours or dollars or machine hours are *volume-driven* or *unit-driven*. They ignore the real causes of costs and simplistically treat all resource consumption *as if* it is a function of volume—as if each unit produced causes an equal increment of activity and thus warrants an equal proration of indirect cost. This *unit-level* treatment may correctly depict the nature of some activities that are performed for each unit, for example, machining operations. But most activities are not at the unit-level. Many activities are done at what is called the *batch-level*, meaning the activities affect batches of product irrespective of the number of units in the batch. The same setup activity that might apply to a batch of five units would also be performed for a batch of 5,000 units. And still some activities are conducted at the *product-level* referring to activities that occur due simply to the very existence of specific product brands or product lines—for example, activities performed by a particular machine or engineer dedicated to a specific product style or line. For example, a given engineer is employed because the company produces a particular type of motor and seeks to make continuous

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improvements in its design. Both batch-level and product-level or product-sustaining activities are clearly not influenced by unit volume, the underlying basis for traditional overhead cost allocation. Their costs can be controlled only by modifying batch- and product-level activities. Then there are *facilities-level* or *facilities-sustaining* costs. These relate to costs of resources that provide general production capabilities such as a plant manager and plant security. These types of costs are less traceable to objects beyond the facility and are unrelated to the number of units, batches, or product types.

Let us return to the illustration above and now use the traditional approach of cost allocation based on direct labor hours. (For the sake of clarity, assume that overhead cost consists only of setup cost.) Assume further that Product A requires four direct labor hours per unit of product and Product B three hours of direct labor per unit. At the same projected volumes of 1,000 and 20,000 respectively for the two products, the total overhead cost of $44,800 would be spread over a total of 64,000 direct labor hours for an overhead rate of $.70 for each direct labor hour. Using this rate Product A would be allocated $2,800 and $42,000 would be allocated to Product B. The traditional approach assigns Product A only about six percent of the total cost due to its low proportion of direct labor hours. Conversely, Product B is burdened with almost 94 percent of the overhead cost simply due to its large volume.

If these cost assignments based on the traditional approach and on ABC are in turn averaged over the total quantity of each product to give average setup cost per unit, the difference between the two approaches is even more dramatic. The setup cost per unit of Product A is $2.80 ($2,800/1,000) using the labor-based allocation; it is $19.20 ($19,200/1,000) under ABC, an increase of 586 percent. Consider the misleading implications of this distortion in product profitability analysis and pricing decisions. Product A is likely to appear profitable when it may be a loser and vice-versa for Product B. The new cost picture might warrant dropping Product A and expanding Product B or even cutting its price. The setup activity could also be targeted for drastic reduction in order to make Product A more viable.

The above illustrations reveal the arbitrariness and inaccuracy of traditional cost allocation. The fallacy in traditional cost allocations is that costs are being assigned based on factors that have no connection to the actual incurrence of those costs. Thus, setup costs have no causal relationship to direct labor hours. Kaplan and Cooper summarize the critical issue in stating that:

> When arbitrary allocations are used, no cause-and-effect relationship can be established between the cost object to which the cost has been

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> assigned and the resources whose cost has been assigned. In an ABC system, every cost assignment to an activity, or a product, service, or customer, should be transparent and traceable, via cause-and-effect relationships, to the demand for resources by the cost object (whether an activity, product, service, or customer). (Kaplan and Cooper 1998, p. 100)

ABC nonetheless has to deal with the fact that most costs of business operations today are fixed. In manufacturing, often the only purely variable costs relate to direct materials and to utilities such as electricity. The issue is not unique to ABC, for even traditional overhead allocation cannot avoid the necessity of applying fixed overhead costs to products. The solution revolves around the determination of the amount of capacity ascribed to the resources assigned to each activity. To illustrate, assume engineering resources cost $280,000 each period and have the capacity to produce 4,000 engineering change orders (the activity driver) each period. The activity rate of $70 per change order would be used to charge different products lines for using this engineering service. If during the period only 3,800 change orders were generated, a five percent reduction in activity levels, then $14,000 of engineering costs would not be assigned to products but would be reported as an unfavorable volume variance and written-off to the period as an expense of waste. Over time, a pattern of this type of underutilization of resources would suggest the need to reduce the resources dedicated to this activity.

THE BENEFITS OF ABC FOR THE MISESIAN ENTREPRENEUR

Entrepreneurial decisions depend upon economic calculation, the "precalculation of expected costs and expected proceeds" and the retrospective "accounting of profit and loss." Decisionmakers expect the information stemming from accounting to present "a faithful image" of enterprise operations. Thus, as mentioned earlier, the costing project conducted in the hosiery division of the large multinational corporation revealed major distortions in the recent profit figures across the division's many product brands. The author observed the presentation of these revelations in a meeting of brand managers who for the first time saw their numbers recast using ABC. Bonus compensation for brand managers in this division is based on the profitability of the brands. The presentation of these new disclosures created an atmosphere of both relief and anguish. The managers of the simple standard brands, which they "knew had been subsidizing" the more complex fashion brands due to volume-based allocations of indirect costs, were understandably pleased. On the other hand, the managers of the fashion brands openly expressed bitter resentment. The information pointed to new directions for brand emphasis and the need for dramatic cost reduction in the fashion lines.

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At a Whirlpool plant with a product line of 333 different refrigerator and vertical freezer models, the managers sought more accurate unit product costs in order to improve "identification of profitable products; competitive analysis; cost reduction; investment analysis; budget development; and make-versus-buy analysis" (Greeson and Kocakulah 1997, p. 16). As a result of an ABC pilot program, they found that their traditional overhead allocations, using only volume-related allocation bases, had *undercosted* low-volume products and *overcosted* high-volume products: the ABC system "allocated two or three times more overhead than the traditional cost accounting system to many of Whirlpool's low-volume products" (Greeson and Kocakulah p. 18).[4] The project also demonstrated that volume was not the only factor affecting product costs—complexity was also found to play a significant role. Not surprisingly, it was discovered that high-complexity products were being undercosted while the traditional allocations were overcosting low-complexity products. "One extremely complex product had a traditional overhead allocation of $65.46 versus $728.92 under the ABC system" (p. 19). (The critical connection between complexity and costs will be explored below.) Additional analysis revealed an interactive relationship between volume and complexity. Most of the products in the low-volume groupings were also found in the high-complexity groupings; conversely, the high-volume products tended to be in the low-complexity classifications.

Whirlpool uses the ABC-generated information in reaching make-or-buy decisions. When capacity constraints arise, outsourcing is no longer simply a matter of splitting the production of components with suppliers. Now more reliable cost data affords a basis for evaluating supplier proposals and choosing "the best candidates for outsourcing" (p. 20). The improved cost data also pointed to opportunities for considerable reduction in cost. For example, having determined that the setup cost in manufacturing a particular component called a spider ring was more than $1,000, a realignment of the machinery reduced costs by more than $12,000 per month.

At Tycos Instruments, a maker of blood pressure gauges and stethoscopes, ABC has enabled senior management to know "which product lines are truly profitable. This information is vital, because to protect a company's core business and expertise, investments must be directed to where they will do the most good" (Drumheller 1993, p. 26). Improvements were also made in make-or-buy

[4] An interesting extreme result was a product typically produced in an annual quantity of one. Under the traditional approach, this product was allocated $64.48 of overhead; ABC assigned a staggering $25,447.11! So much for the significance of decimal places—it's the left side of the decimal that is of concern.

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analysis and cost forecasting. In addition, ABC has altered the company's approach to analyzing capital spending. Prior to ABC, capital expenditure requests were based only on anticipated savings in direct labor. "Now, cost savings throughout the factory are incorporated into the analysis" (p. 26). The estimated reductions in the number of vendors needed or in defects and rejects are factored into the analysis—a focus on *all* cost repercussions.

Like Whirlpool, Tycos also found ABC instrumental in determining the cost of complexity associated with unique as opposed to common parts. Using less unique parts can reduce the cost of inventory as well as warehouse space while realizing economies of scale in procuring raw materials. In designing a certain new product, purchasing personnel at Tycos had pointed out how a cheaper material, i.e., lower priced, could effect substantial cost savings. However, ABC traced the cost of providing manufacturing support, adding a new vendor, and additional receiving and accounting activities that the new part would require. The cheaper part would actually have led to higher *total* costs, a fact unrecognizable without the cost tracing afforded by ABC.

It should be clear that ABC offers not only improved cost accuracy but reveals opportunities for cost reduction. Carrier Corporation, one of the world's largest manufacturers of air conditioning and heating products, found that *complexity* is a dominant cause or driver of its manufacturing costs. Seen as synonymous with diversity, complexity was manifested in expanding product lines requiring more unique parts and processing options. Employing ABC as a key element of a "complexity reduction" initiative, Carrier classifies "all batch and product-sustaining as 'costs of complexity'" (Swenson 1998, p. 21). Financial measures now used to monitor the cost of complexity (COC) include COC as a percentage of overhead, of total product cost, and of revenue.

ABC can be used in transfer pricing where real market price data are not available and other traditional methods are considered unacceptable. Teva Pharmaceutical Industries Ltd., an Israel-based company, reorganized its operations into decentralized cost and profit centers, including an operations division comprised of four manufacturing plants as cost centers and three marketing divisions organized as profit centers (Kaplan, Weiss, and Desheh 1997). For purposes of charging the profit centers for products made in the operations division, traditional transfer pricing approaches based on market price, full cost, marginal cost, and negotiated price were considered and rejected. Inadequate market data precluded a market price-based method. Traditional overhead allocations for full-cost pricing did not reflect the actual cost structure in Teva's plants and also would encourage the marketing divisions to push aggressively more low-volume custom orders while seeking

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less high-volume orders. Using short-run marginal cost would include only the expense of ingredients and packaging materials. This was deemed inadequate for their purposes. And negotiated transfer prices were seen as a source of endless and nonproductive debate among the different division managers. Implementing a system of ABC-based transfer prices using prospective calculations, the market divisions now receive charges based on separate unit-level, batch-level, product-sustaining, and facilities-level costs. These prices in turn altered the decisions of the market division managers in ways that have improved company profitability. For example, producing sample packages of six tablets was now seen as very expensive, making it cheaper to just give physicians the regular twenty-tablet packages. At one point when initial ABC analyses revealed that several of Teva's products were unprofitable, there was still a reluctance to drop these because many of the costs assigned to them, especially the product-level and facilities-level costs, would not immediately go away if they were dropped. But in the early 1990s growing sales volume put a strain on production capacity. The decision was made to sell thirty low-volume products and to shift the product mix away from unprofitable products. This released capacity of people, machines, and facilities for producing new products and increasing sales of existing profitable products.

When a new division of a Fortune 500 company was on the verge of committing $50 million to a "cybermall" in the new electronic marketplace based on a broad strategic analysis with only a few details, the CEO successfully argued for a closer look. An ABC model of the proposal was developed and used for capital budgeting analysis. The resulting forecasts differed significantly from the picture painted by the initial broad analysis. For example, "the ABC model forecasted that an additional $10 million of capital costs would be needed" (Coburn, Grove, and Cook 1997, p. 38). Also startup costs estimates were increased. As a result of the ABC analysis, senior management reversed its initial decision and dropped the idea. In addition to improving the long-standing measurement of product and service profitability, ABC is extending profitability measurement to other objects. Companies can now determine more accurately the costs of serving different customers who make varying demands on the company. Kaplan and Cooper found a company which, after implementing ABC, learned that surprisingly its two most unprofitable customers were in the top three in sales volume (p. 185). Underlying activity analysis revealed that the special processing required to fill the orders received from these customers was unusually costly. By making substantive changes in the manner in which the sales orders were processed, significant

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cost reductions were realized and the customer relationships were maintained rather than broken.

ABC AND TARGET COSTING

Since the mid-1990s another new approach, "target costing," which originated in Japan in the 1960s, has received increasing attention from American companies in reaching decisions regarding new products. Target costing is integrated into the ABC paradigm as a proactive means of effective cost management. Unlike the traditional approach to pricing, target costing approaches costing in a fundamentally different way. Rather than moving from cost to price, target costing starts with the expected market price and works back to determine allowable cost. Thus, for a new product under consideration, the target cost equals the anticipated selling price, derived from market research and analysis of competitors' products, less the desired unit profit. For example, if the competitive price for a given product is expected to be $100 and the company aims for a 15 percent profit margin on sales, then the target cost for this product is set at $85. Analysis is then undertaken to determine the feasibility of achieving this cost level. If indications are positive, then efforts by teams of individuals from all areas of the company are devoted to establishing the specific steps to launch the product without exceeding the total allowable costs. This is a proactive, price-driven approach to costing in contrast to the customary method of cost-driven pricing. Projected costs are compared to projected market prices. Costs are managed before they are incurred; cost minimization is sought ahead of production rather than later.

There are two critical reasons for the development of target costing. First, from the viewpoint of many companies, the anticipated market price is taken as given due to intense competition and the amount that customers are expected to be willing to pay for the new product. The company cannot simply calculate the costs, add in a profit margin, and then expect to sell the product at that desired price. And second is a critical point regarding the nature of costs today: a large proportion of the ultimate *total* cost of a product is committed or "locked in" at the design stage: "Target costing focuses on product design because most costs, nearly 70–80 percent, are *committed* at the design stage, while only 10–20 percent of the costs are *incurred* at this stage" (Ansari et al. 1997, p. 4). In other words, the opportunity to control costs is far greater before undertaking the manufacture of the product than once production is underway and all components and processes have been set.

The relevant expected costs include *all* costs that can be identified across the "value chain," which refers to the various functions that add value in the process of providing products and services to customers. The value chain

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begins "upstream" from manufacturing in engineering design and development. Manufacturing then makes the product in accordance with engineering requirements, and subsequently, "downstream" functions of marketing, distribution, and customer service attend to selling and delivering the product to customers and providing post-sale customer support. Target costing thereby looks at the entire gamut of costs *proactively in the design stage.* This is why "cross-functional teams"—comprised of representatives from engineering, purchasing, manufacturing, cost accounting, marketing, distribution, and customer service—are essential throughout the process from the very beginning.

In many cases the team also includes outside suppliers of product inputs. At one of the world's largest manufacturers of automobile components such as antilock brake systems, ITT Automotive, "key suppliers are considered an integral part of the target costing team. These suppliers should be consulted early in the product life cycle, and they should play a significant role in product design and development" (Schmelze, Geier, and Buttross 1996, p. 26). This close collaboration with suppliers extends the cost reduction process beyond the walls of the company to create what some refer to as the "extended enterprise" (Ansari and Bell 1997, p. 80).

Target costing entails *life-cycle costing,* meaning that "all the costs of owning a product over its life, such as purchase price, operating costs, maintenance and repairs, and disposition costs" are considered (Ansari and Bell 1997, p. 15). This means that the company looks beyond its own costs by also considering the customer's *post-purchase* costs of use and final disposition of the product. Minimizing total costs from the customer's viewpoint provides a competitive advantage. For example, the efficiency and durability of the product affects the customer's operating cost. Product disposal and recycling have increasing cost implications for customers.

The target costing process involves determining specific ways to close the gap between estimated cost under present capabilities and the allowable cost. At ITT Automotive, a cross-functional team undertakes an iterative process of "value engineering" to determine means of cost reduction *without compromising the quality and functionality of the product.* The objective of value engineering is to establish the optimal processes, materials, personnel, and equipment needed in order to achieve the target cost. Activities that do not add value to the product from the customer's point of view, such as machine setups, are candidates for reduction or elimination. Unnecessary complexity, such as excessive parts, is targeted for reduction, as undertaken at Carrier Corporation. If it is determined that the target cost is ultimately unachievable, the

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company has various options including postponement, alteration of the product's features, increasing the target cost, and abandonment.

Activity-based costing (ABC) is a key tool of target costing. Its underlying activity analysis, applied across the entire value chain and beyond to suppliers, can discern those activities that add no value to the product or service and, hence, can be eliminated. Cost estimation is facilitated through cost tracing using the cause-and-effect linkage of activity costs to the product. Carrier Corporation has eliminated "cost-plus" pricing and uses ABC to establish the target cost of a new product under consideration. If present estimates of cost based on the initial design exceed its target cost, "engineers will attempt to either design costs out of the product or improve the manufacturing process" (Swenson 1998, p. 27). If in the end, the target cost is deemed out of reach, the product will not be undertaken. By connecting ABC with target costing, engineers are encouraged "to control costs by designing products that use common components and standard manufacturing processes. Gone are the days when engineers prided themselves in developing unique, elegant parts" (1998, p. 27).

Changing market conditions propelled by phenomenal advances in technology and information flow and decreased barriers to conduct business globally have led to accelerating innovations and shorter product life cycles. With the rush to maintain a constant stream of new products, companies today have a greater need to act swiftly and with more insight into the still uncertain future. For example, Texas Instruments, as a supplier of memory chips to PC manufacturers,

> must understand that the relative price range for PCs in the market will determine the ultimate price it can charge for any improved memory chip. . . . The sharing of experience and technical expertise by value chain members pervades all stages of the target costing process. (Ansari and Bell 1997, p. 81)

At Texas Instruments target costing has been instrumental in one of the largest investments the company has ever made in new product development, revolutionary digital imaging technology. Facing intensely competitive market conditions, "reducing production costs before full-scale production begins is crucial for product profitability" (Dutton and Ferguson 1996, p. 38).

CONCLUSION

The problem of cost is the cornerstone of economic calculation. Entrepreneurs act upon the cost implications of their decisions and depend on cost data that represent the actual pattern of resource consumption. These theoretical generalizations are essential; however, they do not elucidate the complexity of cost

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assignment and how cost accounting is driven to change when cost structures shift and operating segments multiply. Neither do they provide concrete illustrations of how actual decisions are influenced by the available cost information. Cost determinations, whether accurate or not, have their say about the way resources are actually directed in our economy by myriad companies like Whirlpool and Carrier. The need for improved output from cost accounting is a result of changing market conditions. More accurate costing and strategic cost management are now required and made available through advances in information technology. More costs are indirect and not unit-based—that is, more costs are fixed and tied to resources bought in large chunks of serviceability. More sophisticated cost analysis is needed to "slice and dice" cost data across diverse segments or objects. By examining fundamentally new approaches to cost accounting, we see the operation of competitive market forces upon the quality of economic calculation itself. A critical facet of Austrian analysis is elucidated and clarified, showing that economic calculation is not perfunctory or static.

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"RADICAL SUBJECTIVISM": NOT RADICAL, NOT SUBJECTIVIST

JOHN O'NEILL

The running debate in Austrian economics between radical subjectivists and their "moderate" opponents is one that follows a subjectivist script. On one side the radical subjectivists accuse their more moderate counterparts of a failure of theoretical nerve in taking subjectivism to its proper conclusions (Lachmann 1986; Wiseman 1989).[1] On the other side, the moderates accuse the radical subjectivists of abandoning the proper subjectivist insights of Mises and Hayek for a "radically subjectivist nihilism" (Kirzner 1995, pp. 11–24). In this article I argue that the whole debate is misconceived because it follows this subjectivist formulation. I do so by focusing on what is usually taken to be the core claim of radical subjectivism which is taken to define the essence of the position and which underlies its more specific claims questioning the existence of coordinative or equilibrating tendencies within market economies.[2] I show that the core claim of radical subjectivism, as it is presented by its proponents, is neither radical nor subjectivist. It is not radical—the thesis it defends has been around for some two millennia. Attention to that history shows that it need not be subjectivist. In the following I will restate the radical subjectivist position in the logical terms of the older debates. Thus stated the position is not "nihilist" nor irrational. However, neither does it entail the fundamental revision of the aims of economic theory that its proponents often suppose. Whether or not it is true is another question. It is not my main purpose here to either defend or criticize radical subjectivism, although I will raise

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[1]Although not an Austrian, Shackle's (1983, pp. 28–37; 1979) arguments for radical subjectivism have been particularly influential.

[2]My own view is there do exist major discoordinating tendencies within market economies, but for reasons other than those offered by radical subjectivists (O'Neill 1989; 1998, chap. 10).

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problems with the position. Rather my purpose is primarily to clarify the terms of the debate by pointing to the logical implications it has which are lost in its subjectivist formulation. The central problems with what is called the "radical subjectivist" thesis are not addressed by either party in the current debates.

Radical subjectivism presents itself as a radical revision of the epistemic arguments for markets which have been at the center of Austrian economic theory – arguments which I have criticized elsewhere.[3] The epistemic arguments present the market as a solution to problems of ignorance. Two sources of human ignorance are invoked in the traditional Austrian position. The first is what Hayek (1949b,c) calls "the division of knowledge" in society; that is, the dispersal throughout different individuals in society of knowledge and skills, not all of which can be articulated in propositional form and much of which is about particulars localized to a specific time and place. Hence, no single individual or subgroup of individuals within society could in principle possess all the knowledge required for coordination. The market acts as a coordinating procedure which through the price mechanism distributes to different actors that information that is relevant for the coordination of their plans. Central to that coordination is the activity of the entrepreneur who is alert to new opportunities in the market place. Through the activities of the entrepreneur the market acts as a discovery procedure. The entrepreneur is faced with a second source of ignorance, a future that at the point of decision is unpredictable. Two points are invoked here. The first is a form of practical libertarianism. The argument runs, that from the point of view of the actor the future outcome of a decision is indeterminate, since if it was known in advance there would be no decision to make: "[T]o acting man the future is hidden. If man knew the future, he would not have to choose" (Mises 1998, p. 105). Second, there is an argument from the unpredictability of future knowledge and invention for supposing that future human wants are in principle unpredictable. Wants change with the invention and production of new objects for consumption. If one also accepts claim often attributed to Popper that the progress of human knowledge is in principle unpredictable – if we could predict future knowledge, we would already have it[4] – then

[3]While there is a deal of power in the epistemic arguments as negative arguments against particular forms of centralized planning, the epistemic arguments understood as positive arguments for market economics are I believe unsuccessful. I criticize (1998) these positive arguments and the general Austrian case for the market.

[4]See the preface to Popper (1986). It is interesting to note here, however, that the claim is one that was developed from the socialist side of the socialist calculation debates by Neurath: "He who wants to predict a new invention in social life or in engineering has to anticipate this invention. . . . [t]herefore we have to admit that as empiricists we cannot predict changes in our social structure as might predict changes in the astronomical constellation" (1943, pp. 148–54); see also Neurath (1944, pp. 28ff.). This admission that, as Neurath put it in his 1921 paper "Anti-Spengler," action is "an anticipation of unpredictable events" (Neurath

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it follows that since human invention relies on the progress of knowledge, and wants are created by human invention, future human wants are also in principle unpredictable. Hence at any point in time, we are ignorant about the full range of future human wants. The market is presented as a discovery procedure in which different hypotheses about the future are embodied in entrepreneurial acts and tested in the marketplace (Hayek 1978, pp. 179–90; Kirzner 1985). Through such acts market coordination is realized.

While radical subjectivists raise questions about the coordinative tendencies of markets, the arguments for human ignorance from the nature of decision and the unpredictability of future knowledge remain central to radical subjectivism.[5] They form common ground for rejecting the possibility of knowledge of particular future contingent events shared by proponents on both sides of the debate. The thesis that comes under the title of radical subjectivism, however, adds a further twist to the claim that the future is unpredictable. The arguments from decision and the unpredictability of future knowledge, while involving denial of the possibility of knowledge about particular future states of affair, do not entail the denial of the claim that there is truth to be known. Radical subjectivism denies this additional claim. The argument goes that it is not just that the future is unknowable, but that the future itself depends upon current choices. The market is thus not a discovery procedure but a creative process through which the future is made (Buchanan and Vanberg 1991, pp. 187–220). Since the future depends on current choice, there is now, strictly speaking, nothing to know. The indeterminacy of the future is presented by its proponents as the defining core claim of radical subjectivism. Thus Wiseman (1989, p. 230) writes: "The essence of the radical subjectivist position is that the future is not simply 'unknown,' but is 'nonexistent' or

1973, p. 159), as such rules out only those forms of technocratic planning which assume otherwise. As I have argued elsewhere there is a great deal in common between Neurath's criticisms of pseudorationalism and Hayek's case against Cartesian rationalism (O'Neill 1996, pp. 431–42). It is a point that Neurath himself makes in his correspondence with Hayek (Neurath 1945, p. 243). More generally, there is some common ground between Austrian epistemic arguments against centralized technocratic planning and those offered by its socialist critics (O'Neill 1998, chap. 10).

[5]Both points are made succinctly by Shackle:

> Complete prediction would require the predictor to know in complete detail at the moment of making his prediction, first, all "future" advances of knowledge and inventions, and secondly, all "future" decisions. To know in advance what an invention will consist of is evidently to make that invention in advance. . . . *[P]redictability of decisions* . . . is either a contradiction in terms or an abolition of concept of decision. (Shackle 1958, pp. 103–04)

Shackle's points are endorsed by Lachmann (1977, pp. 88ff.). What betrays the radical subjectivist turn here is the use of scare-quotes around the term "future."

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'indeterminate' at the point of decision." The same point is made thus by Shackle (1983, pp. 28–37; 1979): "the content of time-to-come is not merely unknown but nonexistent, and the notion of foreknowledge of human affairs is vacuous." However, thus stated, this core claim is a variation of a position that has been around since Aristotle and it can be restated with greater clarity, stripped of its subjectivist surroundings, in logical terms. A reformulation follows.[6]

Suppose Entrepreneur E is considering whether to launch a new product P. E judges that if over n consumers buy P at £m then the product will be worth launching. E has to judge now at t_0 the truth or falsity of the claim "over n consumers buy P at £m at t_1." On the older epistemic version of the Austrian position this is just a matter of the limits of foreknowledge. At this point in time t_0, the claim "over n consumers buy P at £m at t_1" is either true or false – it has a definite truth value. In logical terms, assertions about future states of affairs fall under the principle of bivalence – that every statement has a determinate truth value, i.e., for any proposition, P, it is either true that P or false that P. Our problem is that in principle we cannot know what that truth value is. Radical subjectivism denies that claim. The problem is not just one of limits to foreknowledge, but the dependence of future states on current choices. The principle of bivalence does not hold for singular statements about future contingent states of affairs. Currently at t_0, prior to the choices of the entrepreneur, E, and his competitors and the choices of consumers which will together determine whether it becomes true, the statement "over n consumers buy P at £m at t_1" has no determinate truth value. It is not the case that it is now either true or false. It needs to be noted that to say this is to deny bivalence and not the law of the excluded middle, "P or not P": "either over n consumers buy P at £m at t_1 or it is not the case that over n consumers buy P at £m at t_1." Now at point t_0, one might say the claim "over n consumers buy P at £m at t_1" is either-true-or-false, but not yet either true or false (Dummett 1978, p. 338). That is what is meant in saying that propositions about future contingent events are indeterminate. They lack a determinate truth value.

Some comments on this position. First, to call it a radical subjectivist position is historically an oddity. It is not "radical" at least in the sense of being a radical departure from older positions. It is a variant of a position in a debate about the relation of future states to current choices that goes back at least as far Aristotle[7] and has a long history in scholastic philosophy concerning foreknowledge and

[6]The following restatement draws on O'Neill (1989, pp. 130ff.).

[7]See Aristotle (1963, chap. 9, pp. 132–42) for an excellent commentary. A clear critical outline of this interpretation of Aristotle is to be found in Kneale and Kneale (1962, pp. 45–54).

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determinism.[8] My statement of the position represents an application of one interpretation of Aristotle's position in *De Interpretatione* chapter 9 to the market context. Aristotle's general contemplating the possible sea battle the following day is replaced by an economic entrepreneur contemplating the possible success of a business project. The position need not be characterized as "subjectivist." The core points are about the reality of the future, the relation of current choices to future states and the status of the logical principle of bivalence. It can be stated without reference to the notion of subjectivity at all and in the traditional discussions from Aristotle through the scholastics it was thus stated.

A corollary of this point is that anyone who defends what is now called radical subjectivism needs to defend those specifically logical claims against possible objections which have their own long history from Cicero (1941, pp. 37–38) to Quine (1953, pp. 65–67).[9] The issues at stake are simply not those assumed in the current Austrian debate. What then is required to defend the radical subjectivist position? First, it requires a revision of logic. It requires the rejection of classical two-valued logic, for some version of a three-valued or many-valued logic.[10] Second, it requires some account of its consistency with current physical theory. In particular, it needs to offer some account of how an indeterminate future is compatible with the theory of relativity. There is an argument to the contrary, that to hold that the future is indeterminate assumes an absolute conception of time. Given the special theory of relativity, if you and I pass each other at very large velocities slicing the space–time continuum at different angles, there are events that are in the future according to my coordinate system but in the present in your coordinate system. Assume some event, say an intergalactic product launch. It is possible that this is in the future in my coordinate system but in the present in yours. Now if this is correct, given the claim that the future is indeterminate, I have to say "the product launch will be successful or has been successful" has no truth value. However, for you the same statement does have a truth value. We cannot be both right and it can't be that just one of us is right since that would make one of us a privileged observer. Hence, the indeterminacy of the future is not compatible with the truth of special relativity.[11] Now

[8]See Prior (1967, pp. 121–22) and Sorabji (1980, esp. chaps. 5 and 7). My own understanding of the issues owes much to conversations with Richard Gaskin (1994, pp. 83–113).

[9]See Quine (1987) for an accessible account of his objections. For a response to recent philosophical objections see Tooley (1997, chap. 10).

[10]The problem of fatalism and future states provides the basis of Lukasiewicz's (1967) development of three-valued logic. For a discussion see Haack (1974, chap. 4) and Wright (1984).

[11]For a development in more detail of this argument that the rejection of bivalence for propositions about the future is incompatible with the theory of relativity, see Putnam (1975). A simpler statement of it is to be found in Quine (1987).

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whether or not that argument is successful I leave aside here.[12] What is the case is that the defender of the possibility of an indeterminate future has to be able to show that no inconsistency is involved. Moreover, the argument will be quite independent of any issues of subjectivism.

However, while there are clearly issues in logic and physics at stake in the debate, when it comes to the implications of the core claim of radical subjectivism for economics paradoxically there is very little at issue at all. In particular, claims about the indeterminacy of the future add nothing to older epistemic arguments about the unpredictability of the future. The belief that they do is founded upon a confusion. The argument for the view that the core claim of radical subjectivism does require a radical revision of economic theory runs something as follows. If the future is nonexistent it cannot be known: "The history-to-come which will flow from men's decisions is nonexistent until those decisions themselves are made. What does not yet exist cannot now be known" (Shackle 1972, p. 3). There is a gulf between the past and the future: "The absolute and eternal difference between the recorded past and unformed future, despite its overwhelming significance for the very stuff of human existence, has been overlooked in our economic theories" (p. 4). Given that the future, unlike the past is in principle unknowable, it follows that economic theory cannot properly deal with the future. Hence Lachmann's (1986, p. 32) conclusion appears to follow: "economists must confine their generalizations to the knowable past."[13] Economic theory must eschew predictions of the future. The whole traditional understanding of the aims and scope of economic theory has to be revised. Thus goes the claim that the unreality of the future does have radical implications for economics. Is the argument a good one?

The argument trades on a conflation of a logical and a substantive point. The logical point is this: an assertion "A knows P" is true only if P is true; but we have it that—for a statement about a future contingent event, P—it is not now the case that P is true or P is false; hence, as far as knowledge of future contingents are concerned, a statement "A knows P" cannot be true. Likewise, if we define a true or correct prediction as one that makes, now, a true statement about the future, since statements about the future are not yet either true or false, it may look as if prediction in economics, indeed in social science generally, also has to go. This logical point lies at the basis of Shackle's position—"what does not yet exist cannot now be known"—and it is this that appears to license Lachmann's radical revision of the aims of economics. The point is again an ancient one.[14]

[12]For a response see Tooley (1997, chap. 11).

[13]For a recent critical discussion of the argument see Hoppe (1997, pp. 49–78).

[14]It is stated succinctly by Aquinas through appeal to Aristotle as a possible objection to God's knowledge of singular future contingents:

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However, this is a logical difficulty that concerns how we should speak. Thus as Sorabji (1980, p. 124) notes one can remedy the point by revising the usual definition of knowledge, "to allow for knowing propositions that will *become* true."[15] This entails that certification of knowledge of future contingents can only come retrospectively, and this marks a difference with knowledge of the past which can be certified in the present.[16] However, it does not entail that knowledge of future contingents is impossible. Given this revision, the question of the possibility of knowledge of particular propositions about the future would be back in the domain of traditional epistemic Austrian arguments which concern not the existence of the future, but rather the nature of decision and the unpredictability of future knowledge. Whether or not one accepts Sorabji's suggestion, when it comes to prediction, the difference is more clearly ultimately one of modes of speech. Suppose the entrepreneur predicts or guesses "over n consumers buy P at £m at t_1" and it comes to pass that over n consumers do indeed buy P at £m at t_1. Given the purely epistemic Austrian position which accepts the principle of bivalence, one can say "the entrepreneur's prediction was true." On the radical subjectivist position this may be, strictly speaking, false. What one should say is that "the entrepreneur's prediction became true." Likewise to say "he guessed correctly" is strictly speaking, false. One has to say "the guess turned out to be correct." However, again these are shifts in modes of expression, not in substantive economic theory.

> Nothing but the true can be known, as is said in the *Posterior Analytics*. But, as is said in *Interpretation*, there is no definite truth in singular future contingents. Hence, God does not have knowledge of individual and contingent futures. (Aquinas 1952)

See also the comments on Alexander of Aphrodisias in Sorabji (1980, p.124).

[15]Sorabji also argues for independent reasons against the acceptance of the Aristotelian position.

[16]A version of Sorabji's move is apparent in the work of Kirzner, who makes a similar move with respect to the certification of error. Kirzner is concerned with possibility of error about the future contingents. More specifically he addresses an argument offered by Buchanan and Vanberg questioning the possibility of error about future contingents: "There can only be 'error' if the future can be known. But if the future is acknowledged to be created by choices that are yet to be made, how can it be known?" Another simpler argument from the non-existence of the future to the impossibility of error that parallels the impossibility of knowledge, might run thus: A is in error believing P only if P is false; for a statement about a future contingent event, P, it is not the case that P is true or P is false; hence, as far as knowledge of future contingents are concerned, a statement "A is in error in believing P" cannot now be true. Kirzner's (1992, pp. 21–26) response to the first argument would also run with the second, is to grant the radical subjectivist's premise – the future is indeterminate – and to grant with it that now, ex ante, one cannot ascribe any error, but to argue we can say in retrospect, ex post, that an error was made. To bring out the parallel with Sorabji's suggestion, our belief that proposition P, an event E that happens at a particular future date t_n, is in error if it becomes the case that P is false – E does not happen at t_n.

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The radical subjectivist conflates points about the logical status of bivalence for assertions about future contingents with a quite distinct set of claims about the availability of well-grounded economic theory and statements about future contingents. Nothing licenses that shift. The substantive question is this: can economic theory make claims about contingent future events or must it confine itself to claims about the present and past? The radical subjectivist has to reject one response to that question; that economic theory can make statements about the future which are, now, true. However, it is quite open for the radical subjectivist to assert that economic theory can make well-justified claims about what will become the case in the future. This involves a shift in verbal expression as to how the question might be answered, but not in substance. The claim that the future is indeterminate does not entail that there cannot be well-grounded beliefs, theories and predictions about how future contingent events will turn out, nor that some theories and predictions about how the future will turn out are not better than others. The rejection of bivalence for statements of future contingents does not entail that there is nothing to choose between propositions about the future, that there is not good reason to believe some forecasts of future state of affairs—shifts in market outcomes, election results, the actions of workers and employers in industrial disputes and so on—and to deny others. Where those beliefs are well-grounded and turn out to be true, it is a verbal dispute as to whether or not we say "the forecast was true" or "the forecast turned out to be true." The rejection of economic theory about the future does not follow from the indeterminacy of the future. Correspondingly, it does not entail that economics can only speak of the past. While there may be other empirical arguments that radical subjectivists make which might entail revisions of economic theory, nothing as such about economics or the aims of economics follows from the core claim of radical subjectivism, that the future is indeterminate.

While in physics and logic important issues do hang upon the indeterminacy of the future, as far as economics goes nothing of substance hangs upon the claim. Radical subjectivism does not have, as far as I can see, any proper implications for economic theory that could not be stated by someone who accepted the principle of bivalence together with the Popperian claim that future states of human knowledge are in principle unpredictable. Thus, insofar as specific claims in economics are concerned, the debate between radical subjectivists and their opponents is an argument about preferred modes of speech.

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AUSTRIAN ECONOMICS AND THE MAINSTREAM: VIEW FROM THE BOUNDARY

ROGER E. BACKHOUSE

As a mainstream economist who is sympathetic to some, but by no means all, Austrian themes, I wish to offer an interpretation of the difference between Austrian economics and the mainstream that differs from some of the ones I have found in the literature. It is my view that mainstream and Austrian economists should pay more attention to each other, but to do so requires a clearer understanding of what distinguishes these approaches.

It is my view that the distinctive feature of mainstream economics, distinguishing it from Austrian economics, is model-building and the important point about models is that they are not the real thing. This may seem a trivial and obvious point but it has been ignored in much of the literature comparing Austrian economics with the mainstream even though, as I hope to show, it has great implications. As examples of this literature I focus on three main contributions: Vaughn (1994), Boettke (1996) and the methodological essays in Rothbard (1997). I choose these three because they are clear and each comes close, in a different way, to the position that I am putting forward.

DISTINGUISHING BETWEEN THE TRADITIONS

Conceptual

Austrian scholars who have sought to clarify the differences between Austrian economics and the mainstream have, entirely correctly, emphasized

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that the vulgar textbook claim that Menger did no more than offer a nonmathematical version of marginalist price theory, is far from correct. This has led many of them to focus on beliefs about the economic world as distinguishing the two traditions. Vaughn (1994, p. 2), for example, argues that methodological individualism and radical subjectivism are the assumptions shared by all Austrians. As constrained maximization is too narrow a framework within which to formalize these ideas, they imply a rejection of mainstream economics. Even Boettke (1996, p. 24), whose position is very close to the one I am offering here, argues that neoclassical economics can be identified with the Chicago program of maximization, stable preferences, and market equilibrium.

In contrast, Rothbard (1997, chaps. 3 and 7) locates the difference squarely in methodology. Austrian economics, he contends, is defined by the method of praxeology. Economics starts not with empirical laws but with explanatory axioms that are known with certainty. The fundamental axiom is that human beings act—they make choices in order to achieve their goals. It follows that the method of Austrian economics is to use thought experiments to derive qualitative laws. These laws are absolutely true, regardless of time and place, and need only to be applied to, not tested by evidence (pp. 130–32).

The thesis I wish to propose is that Rothbard is right to argue that the difference is methodological but that the methodological difference is not the one he proposes—praxeology does not capture the important issues. The key difference between Austrian economics and the mainstream is, I suggest, found in their different attitudes to models. Mainstream economics is about modelling in a way that Austrian economics is not. Though Vaughn (1994, p. 2) is right to see an aversion to mathematics as "a superficial identifying characteristic" of Austrian economics, attitudes toward mathematics point to the fundamental differences. Though this may seem close to the vulgar textbook claim that Austrian economics is simply neoclassical economics without the mathematics, it is, as I hope to show, very different. It is essentially a thesis about the role of models in mainstream economics.

Before developing it, however, there is an important historical point that needs to be made. That is in making my comparison between Austrian economics and the mainstream, I am referring only to the period that follows what Rothbard has called a "profound revolution in the methodology of the discipline" (1997, p. 28). This revolution, which took place between the 1920s and the 1960s, involved the "accelerated mathematization" of the subject to the extent that Rothbard could describe it as having become like a "third-rate subbranch of mathematics," impenetrable to the layman. However, though economics did

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become mathematized, the revolution involved even more profound changes in the structure of the discipline.[1]

Historical

The change that took place between the 1930s and the 1960s is clearly illustrated by the career of Hayek. In the 1930s Hayek was part of the mainstream. By this I do not mean that his economic theories were not distinctive or that they did not form part of an "Austrian" tradition. What I mean is that the nature of the mainstream was such that a plurality of approaches could be encompassed within it. Hence, Hayek could view himself as "part of a broad scholarly community that had progressed beyond distinctions as to school or country of origin" (Vaughn 1990, p. 389).[2] Keynes and Hayek argued vigorously over the cycle[3] and the degree of their mutual incomprehension was sufficient to suggest a Kuhnian paradigm conflict. However, they could not ignore each other. Surveys (e.g., Haberler 1936; Hansen and Tout 1934) took it for granted that both Keynesian and Hayekian theories had to be discussed and that they could usefully be discussed alongside each other.

In contrast, by the 1950s, virtually all discussions of the cycle took place within the framework of the second-order difference equation defined by the multiplier–accelerator model. Theories that did not fit within this framework, including Hayek's, along with many others, were ignored. The diversity of prewar business cycle theory was completely lost. Similar changes took place in microeconomics, the changes here being perhaps best illustrated by the contrast between J.M. Clark's discussion of "workable competition" and the perfect competition of the Samuelsonian "neoclassical synthesis." Even before he moved to other fields, Hayek was no longer regarded as doing serious economics. His work on knowledge, the market, and competition, which could have found a place in the 1930s mainstream, did not fit in.[4] Hayek may have changed but the mainstream had changed even more profoundly.

Enthusiasm for Keynesian economics was part of the reason but, in the longer term, the methodological revolution, with which the Keynesian revolution was

[1]For a broader account of this transition, see Morgan and Rutherford (1998).

[2]Vaughn (1990, p. 389) argues that Hayek could take this view because discussions centered on limited problems such as "how one defined capital or what the role of bank money was in a trade cycle." In the 1930s it was still possible for economists working within a variety of approaches to participate in a common debate. In contrast, by the 1970s the range of approaches that could be accepted into the mainstream discussion of even such limited issues had greatly narrowed. By the 1990s, the range had narrowed even further, excluding even those working with "old" Keynesian models.

[3]See Caldwell (1995).

[4]It was, of course, not just the Austrians who suffered this fate.

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inextricably linked, was arguably more important. Hayek did not fall out of favor because he was not Keynesian (neither are Friedman or Lucas) but because he was perceived to be doing neither rigorous theory nor empirical work.[5] Boettke (1996) views Samuelson's *Foundations of Economic Analysis* (1947) as crucial to this process, establishing the scientific way to do economics. Blaug (1999) places more emphasis on Arrow and Debreu's "Existence of an Equilibrium for a Competitive Economy" (1954). Mathematical economics like this was greeted with incomprehension by the older generation of economists, such as J.M. Clark.[6] Mathematics did more than create the appearance of being scientific and provide an entry barrier—it narrowed the range of issues that could be explored. However, while this movement took hold of the profession in the 1950s, its roots go back to the 1930s. Though using only simple mathematical techniques, Robinson's *Economics of Imperfect Competition* (1933) exhibits many characteristics of the new approach: a narrowing of the questions, with real-world complications being assumed away to the point where the analysis becomes mathematically tractable. What Robinson candidly described as just "a box of tools" became the standard microeconomic fare of introductory textbooks alongside the Hicks-Samuelson interpretation of Keynesian macroeconomics.

CHARACTERIZING MAINSTREAM ECONOMICS

Substantive Assumptions About the Real World

A "static" comparison between two traditions is possible only if there is, common to each tradition, an unchanging set of assumptions—a Lakatosian hard core. The problem is that such hard cores are, as the literature on Lakatos's methodology of scientific research programs shows, very difficult to isolate.[7] Given this venue, I can leave aside the problem of finding an adequate summary of Austrian economics and will focus instead on the problems involved in finding substantive assumptions to which all mainstream economists are committed.

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[5]Friedman is an interesting case for he succeeded in remaining part of the mainstream despite objecting to prevailing methods. See Hirsch and de Marchi (1990) and Backhouse (1995, chap. 11).

[6]See Blaug (1999, pp. 257–58, n. 2) remembers Clark admitting to his class that he could not understand an article by Patinkin that we would expect today's undergraduates to read.

[7]De Marchi and Blaug (1991) is the major source for this. For a response, see Backhouse (1998a, chap. 3). Though it is not central to the present argument, I suspect it is difficult to offer a convincing Lakatosian reconstruction of Austrian economics. Rizzo (1982) outlines the hard core and heuristics of a possible Austrian scientific research program, but he fails to use this to provide a rational reconstruction of the evolution of Austrian economics, so it is not clear how far his characterization of the scientific research program can be defended.

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1. Maximizing behavior: satisfying models and behavioral theories.
2. Stable preferences: there are models of endogenous preferences. Hildenbrand (1994) has even sought to derive the main propositions of demand theory without any of the traditional assumptions about preferences.
3. Equilibrium: game theory has transformed concepts of market equilibrium to the extent that it is not clear what it means to say that economists are, in general, committed to assuming equilibrium.
4. Perfect knowledge: countless economists have explored ways of modelling imperfect knowledge, even where probabilities cannot be attached to outcomes.

Mainstream economics has undermined views that previously seemed well-established.

Positivism

If anything holds mainstream economics together it is not specific assumptions about the real world but a methodological commitment. This is Rothbard's (1997, p. 29) claim. He argues that the reason for the methodological revolution of the 1930s and 1940s was a commitment to a positivist methodology. This explains why mainstream economists have become "locked . . . into . . . absurd" assumptions such as everyone in the market has *perfect* knowledge (1997, p. 122) or that the economy is always in long-run equilibrium (1997, p. 30).[8] There are, according to positivism as portrayed by Rothbard, four steps in the development of scientific knowledge.

1. Observation of empirical regularities or "laws."
2. Construction of hypothetical explanatory generalizations from which these laws can be deduced and, hence, explained.
3. Deduction, from these generalizations, of further consequences that can be tested.
4. Rejection or modification of explanatory generalizations in the light of test results.

The starting point in this schema is a set of empirical laws, not the explanatory axioms that underlie Austrian economics. This approach to economics, Rothbard claims, is flawed for at least three reasons. No one has yet discovered any empirically robust laws (1997, p. 31). Theories can never be tested because it is impossible to undertake controlled experiments where all relevant variables

[8]Boettke (1996, p. 22) makes a similar point when he refers to natural language having been weeded out in the same way that Betamax was gradually pushed out of the market by VHS despite the former's technological superiority.

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are held constant and hence *ceteris paribus* clauses will never be satisfied (p. 30). The act of making a prediction may change the forces at work (p. 31).

This is the point at which I part company with Rothbard. Though mainstream economics has been transformed in the way he suggests, and though "operationalism" (Samuelson) and "positive economics" (Friedman and Lipsey) were important and influential methodological precepts, to view mainstream economics as "positivist" not only oversimplifies but also seriously distorts it.[9] Positivism, with its requirement that theories be grounded in observation statements, is a profoundly empirical doctrine, whereas much contemporary economic theory is profoundly unempirical.[10] Models are not grounded on empirical laws about the economy. Many mainstream economists would be as skeptical about the possibility of discovering robust empirical laws as are most Austrians and would be suspicious of any models based on alleged empirical constants. In macroeconomics this emerges as the requirement that theories have micro-foundations. Furthermore, not only do many economic models not describe real-world situations, they describe situations that could not conceivably describe the real world. The classic example here is the Arrow–Debreu equilibrium model.

It follows that praxeology, as defined above, does not distinguish between Austrian and mainstream economics. Neoclassical economists frequently judge models not by their ability to survive empirical testing but by their conformity with individual optimizing behavior. Though the idea is developed in a very different way, this involves assuming that individuals have preferences and choose the action that leads to the outcomes they prefer. Like Austrians, they refrain from making any quantitative predictions.

Modelling

The main reason why these characterizations of mainstream economics fall down is that they pay no attention to what I suggest is the key feature differentiating contemporary mainstream economics from Austrian economics (and from certain other heterodoxies)—the use of models. Starting in the 1940s, economics became a discipline dominated by models. This is illustrated in Figure 1, which shows the frequency with which the term has been used in three leading mainstream journals. It was virtually unknown before the 1940s but since then has become standard practice. This is a phenomenon that, until recently, even the methodological literature ignored.[11]

[9] I have a similar objection to McCloskey's (1986) arguments about positivism even though he emphasizes different things.

[10] I owe this point to Terence Hutchison.

[11] This does not mean that all the issues related to models were ignored. Many came up in discussing economic theories.

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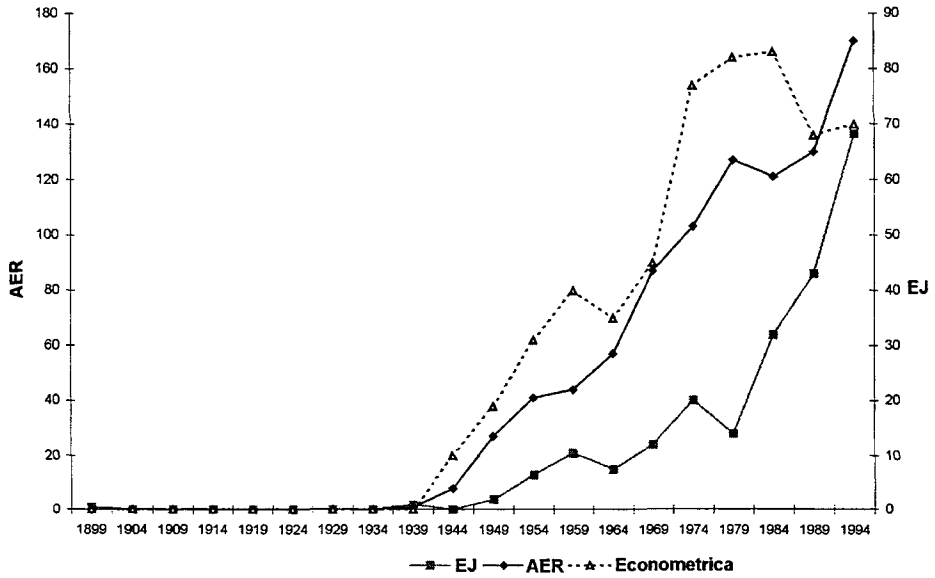
FIGURE 1
THE USE OF THE TERM "MODEL"
IN THREE LEADING MAINSTREAM JOURNALS



Note: This shows the number of articles in which any of the following phrases is used: economic model, mathematical model, empirical model, theoretical model. This method was chosen to avoid picking up articles that used the word model in unrelated senses (as in "the legislation of 1920 was the model for the new deal").

Perhaps the most important point about models is that they are not the same as the real-world phenomena to which they are related. A model railroad may be a working, exact scale model of a real railroad and it is even possible that the track layout and buildings may be faithful models of a specific real-world railroad. A successful model railroad will create the illusion of being a real railroad, and yet no one will ever confuse the two. No matter how accurate the modelling, the differences are clearly too numerous for this to be possible. In the same manner, economic models are different from any realities they might represent. The modeller makes decisions about which dimensions and aspects of the real world are to be represented in the model and which are not. The result is that it is a mistake to assume that economists necessarily believe the models they create. Models are created and analyzed even though their users know that they are, in important senses, unrealistic.

Why do mainstream economists do this? The main reason, which Austrian economists should recognize, is that the economic world is extremely complex.

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General theories are impossible to obtain which means that more limited objectives must be pursued. In choosing the route of constructing models, economists are choosing to analyze artificial worlds that are simple enough for conclusions to be drawn. This, of course, raises the question of whether the models are sufficiently realistic for conclusions drawn from them to be of any use, the Austrian claim being that they are not. The conventional response to this charge is to argue that models need to be tested, which in turn raises questions about how they can and should be tested. The point I would like to emphasize, however, is that the realism of a model depends on the purpose for which it is being used. Take a model railroad. If we use this to ask whether the grocery store can be seen from the top of the station clock tower, it may be correct to assert that the model is realistic, for it can reliably be used for this purpose. However, if we wish to ask about how quickly trains can travel it is not realistic. The same is true of economic models: they may be usable to answer some questions but not others.

Another reason for working with models is a commitment to what is perceived to be rigorous theorizing.[12] Unless we define mathematics extremely broadly, this is not the same as saying, as some critics have, that mainstream economists are committed to using mathematics. It is arguable that economists' primary commitment is to rigor and that they have used whatever tools have been available to achieve this. In the past century this involved what we think of as mathematics, but there is no reason why this should always be the case. However, despite the emphasis on rigor and formal arguments, models have to be linked to reality and here informal arguments are involved. The testing of assumptions and theoretical results can never be completely formal, for judgments have to be made about the relation of theoretical terms to the real world and about the conclusions that should be drawn from the data.[13] Just as important to understanding the way in which models are used, the relationships between different models involve informal arguments and economists' understanding of models and the concepts involved in them evolve over time.[14]

To illustrate the use of models, consider an example: Arrow's influential paper on the allocation of resources to research and development (R&D) (1962).[15] In this paper Arrow constructs a model from which he draws what are

[12]Weintraub (1998) argues that conceptions of rigor have changed, even in mathematics, during the twentieth century.

[13]This is the problem of replication, discussed in Backhouse (1997, chap. 11).

[14]See Backhouse (1997, chap. 10; and 1998b).

[15]This example is discussed in detail in Backhouse (1999).

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very clearly theoretical conclusions—theorems derived from the model. He assumes that investment in R&D produces knowledge; that knowledge, once produced, is a public good the benefits from which cannot be appropriated fully by its producer; and that as a result the production of knowledge will be suboptimal. The formal results refer to the model—to a hypothetical world—not to the real world. However, Arrow also draws conclusions about the real world—about what will happen in a free-enterprise or private-enterprise economy. It is at this stage that less rigorous arguments, inevitably, come in. Three assumptions are particularly important in deriving Arrow's results: conceptualizating R&D as producing a public good (knowledge); interpreting free enterprise as perfect competition; judging efficiency in terms of comparison with theoretical ideal. Though these are clearly fundamental assumptions, they have received virtually no attention in either Arrow's paper or in the large theoretical literature that builds upon this.[16]

To sum up, despite some of the optimistic rhetoric associated with claims for "positive economics" in the 1950s and 1960s, models are not representations of the economy that can be formally tested and applied to derive rigorously-established propositions about the economy. They are tools that have to be used and using tools involves making judgments and engaging in less rigorous forms of argument.

Suggesting that mainstream economists are primarily committed to rigorous theorizing rather than to positivism, or to specific beliefs about how one should model the economy may seem a minor point, but it is very important. It has great implications for trying to understand the way in which economic theory evolves. When economists are finding a strategy to cope with anomalous evidence it makes a big difference whether their primary commitment is to rigor or to the assumption of optimizing behavior.[17] Thus, although mainstream economists may appear, at the moment, to be committed to the assumption of maximizing behavior, if a suitable alternative is offered, this assumption could be abandoned. In addition, if criticisms of mainstream economics are to be effective, it is important that they cannot simply be met with counterexamples that undermine the premises on which the criticisms are based.[18]

[16]This was raised by Demsetz (1969) in a widely-cited article. However, few of the citations to the article take up this point.

[17]For a case study of economists trying to cope with anomalous experimental evidence, see Hausman (1992, chap. 13).

[18]It is tempting to add a third point, that if this interpretation of mainstream economics is correct, it places a question mark over Boettke's (1996) claim that "Austrian economics is historically a school within the broader tradition of neoclassical economics." However, to answer this would take me into a discussion of how far the mainstream can be described as neoclassical and what that term means. Such issues, though interesting, are outside of the scope of this present article.

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CONCLUSIONS

Why Austrian Economists Should Pay Attention to Developments in the Mainstream

Before turning to the question of why the mainstream needs Austrian economics, I wish, in the interests of balance, to offer some thoughts on the reverse proposition – why Austrians should pay attention to the mainstream.

The first reason is a rhetorical one. Austrian economists frequently criticize the mainstream and contrast the two approaches. As the mainstream is evolving, such comparisons need to take account of the latest developments if they are to be effective. Austrians may consider mainstream treatments of time and uncertainty inadequate, but the literature is growing and if it is not taken into account, criticisms will, with some justification, be dismissed. It is, for example, not enough to focus on the Arrow–Debreu model: if the charge that the mainstream does not deal adequately with problems of information and uncertainty is to be effective, the models of Stiglitz, Weiss, Akerlof, and others need to be considered.

The second reason is that even if we accept that economic theories cannot be tested in the way that optimists of the 1960s thought, Austrians have never, it seems to me, explained why models cannot be used for exploring the meaning, coherence and implications of concepts. Boettke (1996, p. 27) writes of Austrian economics pursuing the neoclassical project in "natural language." He may be right to argue that mathematics alone is inadequate for the exploration of "social processes that defy determinate solutions" but no Austrian, to my knowledge, has ever explained why mathematics cannot be used alongside natural-language explanations. On this, my instincts are with Marshall who, though as aware as any Austrian of the problems associated with the use of mathematics in economics, nonetheless believed that mathematics was an invaluable input into the process of theorizing.

The third reason is that mainstream economics does include much work that is relevant to Austrian themes. Anyone interested in market processes should be interested in understanding how individual markets work and hence in, for example, Kirman's analysis of the Marseilles fish market. The data on which such studies are based relate to the actual trades made by real individuals. Some Austrians (for example, Rizzo 1982, p. 58) take it as an axiom that markets are equilibrating. In common with many mainstream economists, I would wish this to be a result, not an assumption.

Why Mainstream Economists Should Pay Attention to the Austrians

Mainstream economics is based on the construction and analysis of models. However useful they are, because they are models, they should not be thought of as fully accurate descriptions of the real world. The problem is that

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economists believe their models. Models should be used but they should not be believed. If mainstream economists were to pay more attention to the Austrians, they would remain more skeptical about their models. I cite three examples.

1. Arrow's analysis of research and development expenditure is important in that it shows one possible way to tackle the problem. The problem is that a literature has developed based on the assumption that it is the correct way to tackle the problem, ignoring other possibilities that may, in practice, be as important.
2. General purpose technologies: models have been developed to show that the introduction of new general purpose technologies may lead to a productivity slowdown. Economists have jumped to the conclusion that new GPTs must always have this effect even though there is evidence that this is not always the case.
3. Real business cycle theory: supporters of real business cycle have shown that technology shocks in perfectly-competitive continuous market clearing models could explain some important time-series properties of major aggregate variables. The conclusion has been drawn that this is the explanation, i.e., that the models are confirmed.

More generally, the problem is that models of competitive equilibrium are more tractable than other types of models, which means that such models are more highly developed than other types of models. Game theory is changing this rapidly, but the problem is recurring here in that models based on manageable solution concepts are being developed most rapidly. The result is that because economists have been able to create such models, there is a tendency to believe that they are an appropriate way to model the world. Economists have a tendency to jump to the conclusion that the models they can construct are correct.

The main reason why, at least at present, Austrian economics is particularly relevant is that it offers a strong challenge to some of the most basic assumptions underlying mainstream models—the assumptions that are so useful that they are most likely to be taken for granted. These include perfect competition, equilibrium, stable preferences, given technology and Pareto efficiency as a welfare criterion.

Lest this seem fanciful, let me give an example that relates to the work of F.A. Hayek. Hayek (1937) raised many questions about the concept of competition. Only two years later, Hicks (1939, p. 83), who must have been familiar with Hayek's work, shied away even from introducing monopolistic elements into *Value and Capital*, arguing that without the notion of perfect competition most

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of economic theory would lie in ruins. He saw no choice but to assume perfect competition. It could therefore be argued that the imperatives of model-building provided the reason why Hicks developed the line of inquiry to which Hayek had raised such profound objections.[19] This takes me back to my earlier point that model-building is the key to understanding the difference between Austrian economics and the mainstream. I remain on the boundary between the two traditions in the sense that while I see a greater role for model-building than do most Austrians, I share with many Austrians the view that there is a great danger from their misuse.

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[19] It should, of course, be added that Hicks not only made important contributions to other ways of doing economics (as in *The Social Framework* and *A Theory of Economic History*) but also repudiated much of his earlier work.

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AUSTRIAN JOURNALS: A CRITIQUE OF ROSEN, YEAGER, LABAND AND TOLLISON, AND VEDDER AND GALLAWAY

WALTER BLOCK

There is no secret concerning the reasons Murray N. Rothbard inaugurated the *Review of Austrian Economics* (*RAE*). It was to promote the Austrian research agenda, and to enhance two-way communication between economists guided by praxeological considerations and their counterparts in the positivist mainstream tradition. Crucial for communication, of course, is publication. But praxeologists were all but frozen out of neoclassical publications. Stated in the introduction to the inaugural issue of the *RAE* (Rothbard and Block 1987, p. xi):

> Articles that simply assume a familiarity on the part of the profession with methodological norms and theoretical developments within the Austrian tradition are unlikely to be published; the profession, by and large, has no such familiarity. Articles that devote substantial space to stating and defending the methodological norms and retracing the theoretical development are also unlikely to be published; they are seen, and correctly so, as unoriginal. Articles whose backgrounds are extensive in absolute terms but brief in relation to the remainder of the article do not constitute a workable compromise; they are rejected on the basis of length. The constraints do not totally preclude the publication of Austrian-oriented articles in mainline journals, but they make such events much more difficult.

A similar vision was adopted by the editors of the *Quarterly Journal of Austrian Economics* (*QJAE*) (Block, Hoppe, and Salerno 1998, p. iii): "to promote the development and extension of Austrian economics and to promote the analysis of

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[1] I speak of the Review of Austrian Economics only during the time I was associated with it, 1987–1997.

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contemporary issues in the mainstream of economics from an Austrian perspective." Both the *QJAE* and the *RAE*[1] are now publishing regularly and have been doing so ever since their respective inceptions. These periodicals have featured pathbreaking research from dozens of Austrians, and have managed to attract the contributions of several notable mainline economists such as Leland Yeager, Richard Timberlake, Harold Demsetz, Gordon Tullock, and Bruce Caldwell. To be sure, the goal "to completely displace the crisis-ridden but entrenched positivism (of the mainstream) by a vital and full-blown praxeological paradigm" (p. iii) has not yet been attained, but it is undeniable that the neoclassicals have been put on notice that their previous unchallenged preeminence is no more.

ROSEN

Judging from several published reactions to this state of affairs, the positivists are not at all pleased with the new Austrian kid on the block. First out of the starting blocks in dismay at this Austrian renaissance was Rosen (1997). Ostensibly an inquiry as to whether there are any gains to be made from intellectual trade between Austrian and neoclassical economics, this is instead a frontal attack on the former school of thought, accusing it of academic superficiality on the ground it has failed the "market test."

Seemingly, there are several areas in which Rosen thinks mainstream practitioners of the dismal science can benefit from praxeological insights; e.g., competition and entrepreneurship (Rosen 1997, p. 149) and information and economic change (p. 150). However, no sooner does he make these overtures but that he takes it all back: "Yet, it is hard to assess exactly what is lost without it [e.g., these supposed Austrian contributions]. The neoclassical approach does not ignore change. Rather, it analyzes change in a more empirically manageable way" (p. 150). The essence of his article can be found in its last section, where he applies "natural section" and "competition . . . to the market for ideas" (p. 150) and asks: "What is the fact that neoclassical economics has scored higher than Austrian economics on the evolutionary/survival test telling us?" (p. 151).

The answer to this query, it would appear, is that Austrians should cease and desist from research and publication. We have lost the market test, have we not? Surely, we must admit, mainline economists are more numerous that praxeologists. Therefore, why don't we play fair, abandon Austrianism, fold-up our tents, and attempt to pass muster under neoclassical auspices. He even comes close to asserting that refereed periodicals such as *QJAE* should discontinue publication: "From the Austrian point of view, there is no dearth of intellectual entrepreneurs looking for heretofore unrecognized opportunities

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to peddle their wares" (Rosen 1997, p. 151). We should do this because we have long ago worn out our welcome:

> The great Austrian economists were recognized as such in their day. The Austrian approach dominated American economics at the turn of the century. They are not forgotten and many of their ideas have been incorporated into the canon. However, others have been forgotten. (p. 151)

In other words, mainline economics has already long since taken unto itself what was true of praxeology, and consigned the rest to the dust bin of history, where it richly belongs. Only spoilsports such as the editors of *QJAE* would continue to promote this school of thought at this late date.

But if anything indicates just how tightly closed-off is the mainline scholarly press in economics to the Austrian School, and thus the continued need for independent journals emanating from this quarter, it is the fact that the *Journal of Economic Perspectives*, the publisher of Rosen (1997), did not call upon a praxeologist to take the other side of this debate, and defend Austrianism against his attack. Instead, they chose Yeager, one of their own,[2] and an avowed opponent of Austrianism on business cycle theory, subjectivism, methodological individualism, the hundred percent backed dollar, the gold standard, time preference, interest rates, and other public policies and methodologies associated with this school of thought (Yeager 1997).

YEAGER

As a presumed defender of Austrianism against the onslaught against this perspective by Rosen, Yeager can hardly be said to have gotten off on the right foot. He implies that all Austrians follow a "party line,"[3] he mistakenly conflates Austrianism and libertarianism, and he sees this school of thought as a "sect" (Yeager, 1997, p. 155). For a person who claims to be a "fellow traveler" (p. 153) of Austrianism, this is all but incomprehensible. How, moreover, is it possible to defend against such a critique when Yeager offers no citations?

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[2]To his credit, Yeager (1997, p. 153) explicitly disavows that he is "a spokesmen for the Austrian school." As well, all throughout his paper, he refers to Austrian economists in the third person.

[3]Because Yeager takes no marching orders, presumably from Austrian central control, he is "free to reject" Austrian "fondness for methodological brooding, pointless profundities and verbal gymnastics" (Yeager 1997, p. 155). Since, in sharp contrast, I am tightly controlled by the praxeological bosses, this permits me to object to this schoolyard type of name calling. Had Yeager attained the usual scholarly requirements, he would have supplied some examples, or citations, documenting this "brooding," etc.; certainly, the disinterested reader might have benefitted from some evidence in this regard. What makes matters worse is that Yeager is presumably defending the Austrian side of this debate vis-à-vis Rosen.

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In addition, Yeager (1997, p. 155) maintains, again without benefit of citation, that some Austrians have wanted to ban mathematics from economics. There are problems here. First of all, Austrians do not reject mathematics out of hand. Rather, while denying its relevance for economic theory, we accept it for purposes of doing economic history. Second, in the ranks of Austrian economists are included distinguished econometricians and mathematicians (Hoppe 1988). So, it is not a case of "the Austrians distaste for mathematics" (Yeager 1997, p. 156), but rather opposition to *misplaced* mathematics. Third, Austrianism, as a praxeological science, is not an inductive but rather a deductive enterprise. As such, it is akin to mathematics, not the physical sciences (Hoppe 1988, 1992; Selgin 1988; Rothbard 1993; Mises 1966; Herbener 1995). How can we oppose mathematics *per se* if Austrianism is but a branch of this endeavor?

Another gratuitous and undocumented attack by Yeager on Austrianism is the claim that "some Austrians go so far as to suspect an affinity between statism and a passion for statistics" (Yeager 1997, p. 156), as if an interest in the intricacies of statistical theory would incline a scholar toward dirigisme.[4]

Nor can we allow to pass without criticism Yeager's attempt to conflate the view of Rosen and Mises (p. 157). The latter does not at all use "*a priori*" in an unusual way, in analyzing the essence of economics. For Mises, economics is not at all a positivist science which apes physics and chemistry in applying empirical methodology to economic issues and facts. Rather, it is akin to the discipline of geometry, or pure mathematics, which rely upon the deductive, not the inductive method (Hoppe 1988, 1992; Selgin 1988; Rothbard 1993; Mises 1966; Herbener 1995).

Yeager's (1997, p. 157) position on the future places him at the extreme Lachmannian-Schakellian (Lachmann 1976) wing of the Austrian distribution: "As for predictions, Austrians take another fact seriously: the economic world is an open rather than closed system and as such has an unknowable future." Nothing in the future is knowable? Hardly. I know that I shall never become the Queen of England, and that Murray N. Rothbard will not be posthumously awarded the Nobel Prize in economics.

Despite the fact that Yeager is no Austrian, and seems to bear a great enmity to this school of thought and its practitioners with his incessant name calling, his (Yeager, 1997) reply to Rosen (1997) still functions as a good antidote to the latter. Of great value is Yeager's criticism of Rosen's analogy between ideas on the one

[4]Advocacy of government-forced collection of statistics, as is done by the Bureau of Labor Statistics, or the Commerce Department, or coerced census reporting of information not called for in the Constitution, is, of course, entirely a different matter. See on this Rothbard (1997a), Van Creveld (1999).

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hand and goods and services on the other. States Yeager (1997, p. 159) of Rosen: "He evidently holds it against the Austrians that they do not pass his market test in the intellectual atmosphere created by members of his own camp, an atmosphere pervaded by narrow yet tacit methodological preaching." Yeager (p. 161), moreover, is perfectly justified in plaintively asking "Since when, anyway, was the market, even the actual business market, the arbiter of excellence in consumer goods, literature, art, music, science, or scholarship? Since when does the market decide truth and beauty?" Surely rap music sales vastly outstrip those of baroque music, and Puff Daddy is far more popular than Mozart. According to the "market" criterion adumbrated by Rosen, we would be forced to conclude that mere popularity, mere purchase levels, are an unambiguous indicator of beauty. *The National Enquirer* outsells the *Wall Street Journal*; presumably, according to Rosen, there is more truth in the former than in the latter.

But the fact of the matter is that Rosen is committing a category mistake. He is conflating scientific fact, and truth, on the one hand, with profits and sales on the other. He is attributing to markets that which properly belongs to the realm of truth or falsity.

Yeager states:

> Scholars should approach each school (and each sect) for what they can learn from it, not as a target of polemics for polemics' sake and not as a foil for self-congratulation. The neoclassical and Austrian Schools, each stripped of excrescences, are complementary. Aspiring Austrian economists should indeed take the standard Ph.D. courses. (1997, p. 164)

But where are the "excrescences" in praxeology? Where are the "polemics"? Further, if there is as much "complementarity" as Yeager claims, why does he content himself with only the claim that "aspiring Austrian economists should indeed take the standard Ph.D. courses"? Why is this not coupled with the obverse, namely, that aspiring neoclassical economists should take the standard *Austrian* Ph.D. courses? The two-way street advocated by Yeager, curiously, runs in only one direction.

LABAND AND TOLLISON

The major thesis of Laband and Tollison is that there is no warrant for the existence of any academic, scholarly refereed journals devoted to the Austrian perspective. The reason that "so-named Austrian economists" (Laband and Tollison 2000, p. 45)[5] fail to "publish in the regular journals of economics . . . is that

[5]Presumably, we are to infer from this that there really is not a core Austrian perspective worthy of the appellation "Economics" without the "so named," and worthy of

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their work is flunking an important market test." In other words, whether real or only "so named," Austrians are purveying a particularly inferior brand of economics. The mainstream journal editors and referees see through the fallacies[6] of praxeology, and, not taken in by them, reject submissions from this dubious quarter.

And how do the Austrians react to these eminently justified rebuffs? Do they go back to the drawing board, rethink their misbegotten positions, and return with a higher quality (e.g., more mainstream) product? They do not. Do they realize that "even the best and brightest among us usually fail. To succeed as scientists we must come to grips with rejection and failure to convince others" (Laband and Tollison 2000, p. 45)? Again, unhappily, no. Do these Austrians incorporate the fact that "sometimes the failure is temporary, overcome by redoubled effort to prove one's point. Sometimes the failure is permanent and the putative contribution must be discarded as either incorrect or insubstantial" (p. 45). Not a bit of it. Very much to the contrary, they engage in "general grumbling"; they indulge in "sour grapes" (p. 45) and "crybabyism" (p. 47). If they had any sense, the Austrians would regroup, and realize that the negative judgment made of them by their intellectual betters is a valid one.[7] They would keep trying to "convince nonbelievers" on the editorial boards of mainstream economic journals even though "this is difficult." And why should Austrians do this? Because this is the only way "to make contributions."

Here, in a nutshell, is Laband and Tollison's advice to Austrians: knuckle down, shape up, get real, avoid the evils of crybabyism, grumbling, and sour grapes; but above all, never never, never start your own journals. For to do this is to leave the intellectual battlefield. They state (p. 45):

> Imagine a young tennis player who complains that he cannot win his matches because the linesmen call too many of his shots out. Sure, the linesmen make mistakes, but does anyone think that the best players are not playing in the major tournaments? It is one thing to win the Austrian Open and quite another to win Wimbledon.

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distinction from that of any other school of thought. But were this true, why are Laband and Tollison castigating Austrians for inaugurating journals devoted to this topic? If there are no real Austrians, but only "so-named" ones, why the fuss on the part of Laband and Tollison over a "so-named" Austrian journal? As a nonreal journal, it should hardly give them offense.

[6]But Austrianism is more than merely incorrect. For Laband and Tollison, too, this school of thought is composed of "true believers" (Laband and Tollison 2000, p. 44) and leads a "basically cult like existence" (p. 43).

[7]But not a perfect one. Laband and Tollison (p. 45) concede that "on occasion . . . (the neoclassical) vetting process is subject to type I error."

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In the event, however, the Austrians have eschewed this counsel. While still continuing to devote a part of their time and effort to publishing in mainstream journals (following Rothbard's advice about "two way streets") they have also had the temerity to start up and contribute to refereed periodicals open to their own perspective.

This is neither the time nor the place to take on the major challenge of Laband and Tollison: that all of value in Austrian economics has long since been incorporated by the neoclassical school; that what remains the sole provenance of praxeology is all wrong; that there is nothing both true and original in praxeological economics as currently practiced. Much of the research and publication in this tradition has been devoted to blazing new and important paths (e.g., appraisement, information, fractional-reserve banking, subjectivism, socialism, property rights) and to a panoply of criticism of the mainstream showing that its analysis in great part is either misguided or pernicious.[8] Instead, I shall critically examine Laband and Tollison's case against the very existence of Austrian scholarly journals.

Let us begin with their tennis analogy. My contention is the Austrians are not poor players of this game, called down for their errors by very accurate albeit imperfect linesmen. Rather, Austrians are engaged in baseball, and don't even make it to first base since tennis has no umpires capable of distinguishing a ball from a strike. While there are indeed two schools of economics, competing with each other in seeking truth in terms of explaining and understanding economic issues, the two are very different enterprises. They have different rules, criteria of success, methodologies, literatures, histories, etc. To ask one to abide by the rules of the other would be to put the former at a great disadvantage.

But this is a two-way street. Yes, Austrians have great difficulty publishing in mainstream journals. However, the same undoubtedly applies when neoclassicals submit articles to praxeological journals. In point of fact, however, it is undoubtedly the case that non-Austrians are represented in praxeological journals to a far greater extent than is the reverse.[9]

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[8]If Laband and Tollison wish to enter the lists and debate praxeologists on any of these specifics, they will be following in the path of several eminent neoclassical economists who have already done so. For recent criticisms of Austrians by neoclassicals see Tullock (1988), Demsetz (1979, 1997), Yeager (1987, 1988, 1989, 1994, 1997), Timberlake (1987, 1988, 1989), Caplan (1999), Nozick (1977). For Austrian rejoinders to Tullock, see Salerno (1989); to Demsetz, see Block (1977a, 1995, 2000); to Yeager, see Block (1988), Barnett (1989b) and Salerno (1994b, 1995); to Timberlake, see Rothbard (1988, 1992) and Barnett (1989a); to Caplan, see Block (1999) and Hülsmann (1999); to Nozick (1977), see Block (1980). Unhappily, Laband and Tollison write as if this literature did not exist.

[9]See the literature cited in the previous note.

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Another difficulty with the Laband and Tollison position becomes clear when we attempt to generalize. That is, let us search around and see if we can uncover *other* schools of thought beside the Austrian which have set up journals of their own, in order to escape from the judgment of tennis linesmen to whose tender mercies they would otherwise have been consigned. Exhibit A in this regard is surely the Public Choice School. These scholars have had the audaciousness not only to create the journal *Public Choice*, but several others as well, including *Constitutional Political Economy, The European Journal of Political Economy, The Journal of Public Finance and Public Choice,* and *The Journal of Public Economics*. Public Choicers approach questions of politics from an entirely new and different methodology (economics) compared to what has gone on with this field before their advent. That is, previous to the arrival of Buchanan and Tullock and their numerous colleagues, such issues had long been addressed using the method of political scientists. In order to be logically consistent, Laband and Tollison would have to upbraid the Public Choice School for starting their own journals. Instead of sour graping and "chatting" (Laband and Tollison 2000, p. 6) among themselves,[10] these scholars should have subjected themselves to the righteous judgment of the editors and referees of political science journals.

I mean this as no criticism of Buchanan and Tullock.[11] But had they allowed political science journal editors to determine the fate of their school of thought it is entirely likely that it would be far less developed than it is now. For the "tennis linesmen" of political science are no more fair to public choicers than are those of mainstream economics to Austrians.

But this is the mere beginning of the troubles for Laband and Tollison on this ground. It is only the veritable tip of the iceberg. Exhibit B might be the work of Gary Becker. Much of his intellectual contribution was to explore questions hitherto in the domain of sociology.[12] He did so, of course, with the tools of economic analysis, not sociological analysis. Had he submitted his unpublished works to the editors of sociological journals, is there anyone rash enough to believe, as Laband and Tollison imply, that they would have been judged by fair and competent "linesmen"? It is of course impossible to say for

[10]In addition to setting up journals, both Austrians and Public Choicers have also conducted annual meetings of their own in competition with the ASSA.

[11]For criticism of the Public Choice School from an Austrian point of view see Rothbard (1997b) and Block and DiLorenzo (forthcoming).

[12]See in this regard Becker (1957, 1964, 1974a, 1974b, 1975, 1976, 1978a, 1978b, 1980, 1981b, 1984, 1990a, 1990b, 1990c, 1991, 1993, 1996). To modern economic sensibilities, this very large literature (I confine myself only to a sampling of his books, for brevity) sounds like economics, not sociology; but especially when Becker began his career, and even nowadays, his work was widely seen as "poaching" on the preserves of sociology.

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sure, but it is extremely likely that had Becker followed the path advocated by Laband and Tollison, he would have enjoyed far less publishing success than he did.

But, again, this phenomenon is ubiquitous; it transcends any one individual, such as Becker, or even any one school of thought, such as Public Choice or Austrianism. To begin with, it applies to every single solitary subdiscipline of economics. Consider labor economics, as but one case in point, by consulting the *Journal of Economic Literature* listings for subfields in economics, or by perusing only the titles of the hundreds of actual economics journals (Cabell 1998). Why create the *Journal of Labor Economics* (*JOLE*), or the *Journal of Labor Research* (*JLR*) or any of the other prominent mainstream scholarly periodicals specializing in labor economics? According to Laband and Tollison (2000), if we generalize from these authors, the editors, referees and contributors to *JOLE, JLR*, and their many counterparts should have, instead of inaugurating their own periodicals, tamely submitted to the judgment of the "tennis linesmen" of the *American Economic Review* (*AER*), the most prominent economics journal of them all. From whence sprang the sheer audacity of *JOLE, JLR*, etc., in rebelling against the just (albeit imperfect) rule of the even more prestigious *AER*? In other words, if *QJAE* is illicit because it is an attempt to evade the just rule of the economics profession as a whole, then this goes, too, for *JOLE, JLR*, and all the rest.

And it goes in spades. For the journals devoted to labor economics (ditto for international trade, economic development, history of thought, economic history, money, macro, etc.) at least share a common logical positivist methodology with *AER*. Far less reason, then, for *them* to break away from the just authority of the "tennis linesmen" of the *American Economic Review* than for Gary Becker, Public Choice or Austrianism to seek to escape the rule of those with whom they do not share a methodology.

Again, we have only begun to scratch the surface of the logical implications of Laband and Tollison (2000). For if their message applies to schools of thought within economics, if *none* of these other journals were justified in opening their doors, the implication is that there should only be *one* economics journal, namely the *AER*. But why should there be *any* economics journals at all? Economics, after all, is but one discipline out of many in the social sciences. Surely, using the "logic" of Laband and Tollison, there were others before economics. The dismal science broke away from these others, presumably, due to the same need for specialization and division of labor which now accounts for the separation between neoclassical and Austrian economics. But if the latter is not justified, then neither is the former. Within social science, journals from the

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various disciplines of sociology, political science, history, anthropology, psychology, etc., should all coalesce into one big social science journal. And if the case for a journal articulating the vision of these traditional social science disciplines is thrown into question by Laband and Tollison, how much weaker, even, is the case for specialized journals for the newer social science subdisciplines such as "feminist" studies, "queer" studies, "black" studies, "transgendered" studies, "multicultural" studies, etc.

As a matter of fact, the logical implication of Laband and Tollison is not only that there should be but one economics journal, not only that there should be but one social science journal, but that there should be only one scholarly publication, period. For, why should the chemists be able to escape the proper authority of the physics "tennis linesmen"? Why should the biologists be allowed to rebel against their proper lords and masters, the chemists? And why should any of these disciplines, let alone economics, be allowed to kick over the traces initially forged by the *first* discipline, the "Queen of the Sciences," that is, philosophy. Chemists, too, are just a bunch of whiners, who "chat" amongst themselves, indulge in "sour grapes" and "cult-like" behavior.

A word on Laband and Tollison's claim that their view represents the "free market" and that the opposite perspective, the one which supports the existence of such journals as the *QJAE* is characterized by "market failure." This contention must be rejected on several grounds. It comes with particular ill grace from authors who argue for, in effect, only one economics journal (and in the extreme, the publication of only one periodical in total). This is incompatible with the neoclassical economics interpretation of single sellers of items being "monopolists." On the one hand, the logical implication of Laband and Tollison, as we have seen, is that there should be only one publication; on the other, it is a well-established neoclassical claim that one of practically anything is equivalent to "monopoly," which, in their lexicon, constitutes a "market failure." The point is that it is their *own* perspective, not that of the Austrians, which is compatible with market failure. And, in turn, it is the Austrians, not the mainstream economists, who have done more than anyone else to undermine the intellectual credibility of the concept of market failure (Rothbard 1962).

Since when, moreover, is it an example of antimarket activity to set up a new business venture? Surely this is an example of entrepreneurship, something fully recognized at least by Austrians (Kirzner 1973), if not neoclassical economists, as an integral aspect of business enterprise. Presumably, if Laband and Tollison were analyzing not academic publishing but rather ordinary

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commerce, they would have stood with the butter interests which tried to prevent the colorization of margarine, with the horse and buggy industry which tried to stifle the early horseless carriage firms with a welter of restrictions. Laband and Tollison are in the tradition of the medieval guilds which long succeeded in preventing competition against themselves.

As a young Austrian economist, I wondered for many years how the academic establishment would respond to Austrian criticisms, when and if they ever condescended to recognize this literature. Although there have indeed been substantive criticisms[13] penned, a substantial part of the response, exemplified by Laband and Tollison (2000) and Rosen (1997) is the *ad hominem* dismissal of praxeology on the ground that there are far more adherents of the logical positivist paradigm than there are of praxeology.

VEDDER AND GALLAWAY

Vedder and Gallaway (2000) have written a very interesting and informative paper on the issue of whether journals which specialize in Austrian economics are justified. They answer this in the affirmative (p. 13): "Since historically the discrimination against Austrian scholarship has been high, a new Austrian academic infrastructure has emerged. . . . Institutions evolve to meet needs prompted by human action."

Their analysis is problematic, however. One difficulty is that Vedder and Gallaway unnecessarily concede to the very editors they accuse of bias against Austrians a modicum of legitimacy. They state (p. 1): "presumably the rise in the last generation of Austrian economics' market share" in the academic milieu reflects mainly the empirical fact that Austrian concepts have proven useful and relevant in explaining contemporary human action." But if *increases* in Austrian market share stem from the explanatory use and relevance of the contributions of this school, then so does the *level* of its market share arise from this quarter. However, according to Vedder and Gallaway's Figure 1, this market share is pitifully low, barely above 3 percent. I agree with Vedder and Gallaway that this is a roughly accurate estimate of the proportion of economists now practicing who could be characterized as Austrian. But this means, then, that the praxeological share of correct analysis of contemporary human action is also at about this level. And that is a claim I find very much incorrect.

Another problem arises with their statement that the "leading economic journals . . . are mainstream forums" (p. 34). In my view, the leading economic journals are the Austrian ones. To be sure, I have no objection to the main subject of their query, to wit, what is the case for and against praxeologists

[13]See note 8 above.

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publishing in the leading *mainstream* journals, only to deprecating the Austrians journals vis-à-vis the *American Economic Review* and its ilk.

A third difficulty arises with regard to their claim that the Austrian is complementary to, rather than rivalrous with, the other free-market schools of thought such as the Chicago, the Public Choice, and the New Institutionalist (p. 35). This impacts, also, their problematic Figure 2.

Imagine a Venn Diagram, with two overlapping circles indicating, respectively, the Austrian views (A) and those of the Chicago, Public Choice and New Institutionalist Schools (B).[14] We further label as X the views which are unique to praxeologists (e.g., which are not shared by the other free-market schools of thought), as Z those perspectives which are unique to the Chicago, Public Choice, and New Institutionalist Schools (e.g., which are rejected by the praxeologists) and as Y those positions which are shared by both camps. What are some examples for each of these subcategories? X contains the praxeological finding that economics is a deductive system, methodological individualism, the understanding of competition as rivalry, not based on the number of participants in a market but rather on whether entry is legal, and alternative or opportunity costs on a subjective basis; on a public policy level, adherence to a gold standard, a 100 percent backed dollar (no fractional-reserve banking), opposition to the concept of market failure, fixed exchange rates, rejection of all antitrust legislation and advocacy of laissez-faire capitalism. In very sharp contrast, Z would include interpersonal comparisons of utility, objective costs, logical positivism, the idea of competition as based on the number of competitors in an industry, acceptance of the logical coherence of indifference curves, and the notion that economic laws must be tested by empirical evidence; on a public policy level, there would be acceptance of at least some antitrust (e.g., antimerger, price fixing provisions), advocacy of fiat currency and fractional-reserve banking, flexible exchange rates, some sort of limited welfare state (e.g., negative income tax, school vouchers, tradeable emissions rights). What are the areas of agreement in Y? That an increase in the money supply (not the "greed rate") accounts for inflation, the median voter analysis, the importance of markets, freely fluctuating prices, institutions, and equilibrium tendencies; on a public policy level, that rent control, minimum wages, unions, tariffs, central planning, government ownership or regulation of airline prices, nationalization of newspapers, steel mills, and oil companies are disruptive to an economy.

With this introduction, we are ready to tackle the claims of Vedder and Gallaway (2000, p. 36). Can these authors be fellow travelers of "both the

[14] Although there are of course differences amongst these three, for present purposes of distinguishing them all from the Austrian perspective, I in effect assume these away.

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Austrian and Chicago Schools simultaneously"? Yes, if they completely eschew both X and Z, and embrace Y and Y alone. Then and only then can they be fellow travelers of both. But to the extent they accept X they must reject Z, and of course *vice versa*, since these elements are contrary to one another. Logically Vedder and Gallaway can reject both X and Z, but cannot accept both of them.[15] What if Vedder and Gallaway agree with Y, but also with some parts of X and others of Z. Then they are partially Austrians, partially neoclassicals, and somewhat internally self-contradictory; i.e., they are eclectic.

The next mistake of these authors (p. 37) is their claim that Gordon Tullock when he edited *Public Choice* and James Bennett, the present editor of the *Journal of Labor Research* exhibit no discrimination against publishing in their journals "Austrian ideas." While I have no doubt this is true of Austrian ideas in category Y (which *also* include neoclassical ideas), I defy anyone to show me the publication of Austrian ideas in category X (which are decidedly *not* compatible with those of the mainstream) in either of these journals.

I entirely agree that Hayek and Kirzner "have been more accommodating of non-Austrian beliefs" than either Mises or Rothbard (p. 37). Hayek has made numerous compromises with doctrine which can be categorized as violative of X, and even Y (Block 1996), while Kirzner (and Mises, too, in this one case) has opposed the total elimination of antitrust (Block 1977b). But none of these economists ever published anything he knew to be untrue, much less with the goal of enhancing his career. Nor can it be denied that the award to Hayek of the Nobel prize in 1974 heightened interest in Austrian economics. However, I date the modern rise of praxeology a decade earlier with the publication of several of Rothbard's books and articles, particularly Rothbard (1962). In any case, the version of Austrian economics most directly furthered by Hayek's prize was a watered down interpretation of the praxeology of Mises and Rothbard.[16]

I especially appreciate Vedder and Gallaway's (2000, p. 37) application of the median voter phenomenon to referees of mainstream journals to explain why Austrians have an even more serious obstacle placed in their path for obtaining an acceptance than otherwise would have been supposed.

Consider now the case posed by Vedder and Gallaway (p. 38) of the author of an Austrian paper on the Mises–Hayek trade cycle theory, whose editor

[15]If Vedder and Gallaway sign on to both X and Y, they are Austrians, pure and simple; if Y and Z, then they are members of the Chicago, Public Choice, or New Institutionalist Schools.

[16]The "dehomogenization literature" blazed by Salerno is of relevance here. On this see Salerno (1990, 1993, 1994a) and Block and Garschina (1996).

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demands econometric evidence. Would it be a violation of Austrian principle to comply? In one sense, of course it is, since empirical work is considered by praxeologists to illustrate, not test, apodictic theory. That is, regression analysis is a legitimate part of Austrian economic history, but this does not hold true for theory.[17] However, it is by no means necessary to advise the editor about this complication. All the author need do, if he judges it worthwhile, is to provide what he considers an empirical illustration of the principle. If the editor wants to interpret this as evidence of relevance to the Austrian business cycle theory, that is his business. However, if this editor demands that the Austrian author characterize this illustration as evidence, then all bets are off. We have now moved into the thickets of area Z. If the praxeologist publishes on this basis, he is indeed selling out in an unprincipled manner.

I also have problems with Vedder and Gallaway's (p. 39) version of the publication process of their project "Out of Work." They state "Rothbard (1983) confided in us that he did not like the double-blind refereeing system, but was being coerced into following it to demonstrate the presence of scholarly standards." This is entirely incompatible with my own recollections of this issue. As coeditor with Murray Rothbard of the *Review of Austrian Economics*, I can assure the reader that Murray took on the task of inaugurating this journal on a double blind refereeing basis completely voluntarily. Those who know him know full well that, apart from the state of course, Murray was never "coerced" into doing *anything* in his entire adult life. Last, they imply (p. 41) that regression analysis is "mainstream" methodology, i.e., a part of Z not, Y. But this is not necessarily so. It is true if and only if the econometrics is used to *test* praxeological discoveries. But regressions can also be used to *illustrate* theory, in which case they fall into category Y, not Z, which is claimed by both schools of thought, not by the neoclassicals alone.

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THE DRAMA REVISITED

LUDWIG VAN DEN HAUWE

THE HAYEK-KEYNES DEBATE: LESSONS FOR CURRENT BUSINESS CYCLE RESEARCH. BY JOHN P. COCHRAN AND FRED R. GLAHE. LEWISTON, N.Y.: EDWIN MELLEN PRESS, 1999.

THE TIMELINESS AND CONTINUING RELEVANCE OF AUSTRIAN BUSINESS CYCLE THEORY

Careful study of the book under review is a rewarding experience. As Roger Garrison points out in the Foreword (pp. iii–vii), Austrian capital-based business cycle theory has lost nothing of its relevance and timeliness. The theory identifies monetary mismanagement as a major source of economywide distortions in the intertemporal allocation of resources by focusing on the relative-price effects—and the corresponding quantity adjustments—of a monetary disturbance, as compared to tracking the movements in macroeconomic aggregates that conceal those relative-price effects. It thus gives us a superior understanding of the real coupling between the short-run and the long-run macroeconomic pictures and of the nature of business cycles. Despite the book's title, and although the authors treat Keynes's ideas not unsympathetically, the outlook adopted in the book is Hayekian rather than Keynesian and the authors' thesis is basically that "Hayek was right" (p. vi).

Substantive theoretical and methodological insights are intertwined in the book. In their discussion of the use of the equilibrium construct or the method of praxeology, the authors do not hesitate to combine citations from both Mises's and Hayek's writings (see chap. 11, pp. 151–67). Surely this approach will appear disputable to at least some readers. Recently several authors have argued in favor of a dehomogenization of the respective contributions of

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Hayek and Mises to economic theory (Salerno 1993; Block and Garschina 1996). Cochran and Glahe do not situate their work as belonging to this trend. Although they rely primarily on the original writings of the two leading actors in the *drama,* they use the writings of Mises and Rothbard as additional primary source material on the Austrian approach to economics, monetary theory, and the nature of the business cycle (p. xiii).

The book brings together sources that to some Austrians may appear hardly compatible, if not inconsistent. Insiders know that there are some significant differences between the views of, say, Mises, Hayek, and Lachman, even with respect to method and methodology. However, the book integrates these different Austrian sources into a relatively coherent picture. The fact that the authors do not want to enter in any depth into issues presently under debate within the "Austrian" School itself may be explained by the fact that they intend to address their book to the economics profession at large rather than to the inner circle of convinced Austrians. The drawback of this strategy is that the reader will not find in the book answers relating to questions that have been intensely debated recently within Austrian economics itself.

One such controversy relates to the appropriate use and interpretation of the equilibrium construct and, in particular, to the question concerning whether or not the market harbors a tendency toward equilibrium. How should the equilibrium construct be used and interpreted?

As Cochran and Glahe state at the beginning of chapter 11 (pp. 151–67) they intend equilibrium to mean the equilibrium of the economic system in the sense in which it is used in Lachman (1976, p. 151, n. 123). It must be reminded that Lachmann essentially viewed Hayek's 1937 essay as "an attempt to set Mises straight" (Hayek 1937; Selgin 1990, p. 35). Lachmann broadened Hayek's thesis, however. Within Austrian economics he represents the skeptical position that no tendency toward equilibrium exists in markets. The particular problem emphasized by Lachmann is that of "divergent expectations" (1976, p. 129). According to Lachman, expectations are autonomous. "We cannot predict their mode of change as prompted by failure or success" (p. 129). The notion of general equilibrium is to be abandoned, but that of *individual equilibrium* is to be retained at all costs (p. 131).

Nevertheless, that is, despite their Lachmannian starting point, Cochran and Glahe go on to point out that the special use of equilibrium analysis employed in Austrian business cycle theory consists of: (a) setting up the conditions existing in the economy at any particular time; (b) setting up the equilibrium conditions for the same economy; the conditions required for compatibility of all entrepreneurial plans; (c) compare (a) with (b). If the two

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are identical no changes can be expected in the economy until the data change. If the two differ, the current conditions cannot continue. Changes can be predicted. The equilibrium conditions can be a useful guide for impending change. The comparison of the fictitious equilibrium state to the existing state allows the economist to proceed from the diagnosis of the existing state of affairs to a prognosis of what is likely to happen in the future (pp. 79 and 161). In the Austrian business cycle theory an equilibrium concept is used not to describe an actual state of a real economy, but to indicate directions of change (p. 157).

Moreover, this interpretation may seem somewhat questionable from a Misesian viewpoint, in particular as regards the use of the equilibrium construct for the purpose of predicting directions of change. On page 157 of the book the authors quote Mises saying that "The static method, the employment of the imaginary construction of an evenly rotating economy, is the only adequate method of analyzing the changes concerned" (Mises 1998, p. 249). Mises mentions the *ceteris paribus* condition where he clarifies that:

> There is no means of studying the complex phenomena of action other than first to abstract from change altogether, then to introduce an isolated factor provoking change], and ultimately to analyze its effects under the assumption that other things remain equal. (Mises 1998, pp. 248–49)

According to Mises "The assumption *ceteris paribus* is the self-evident appendage of every scientific doctrine and there is no economic law that can dispense with it" (Mises 1981, pp. 151–52). Lewin has correctly characterized the evenly rotating economy as "an overriding *ceteris paribus* device" (Lewin 1997, p. 152). It is adopted in order to impose the most general type of *ceteris paribus* conditions (p. 154). But as Mises himself makes clear "these other things are never equal" (Mises 1998, p. 543). Nobody ever was or ever will be in a position to observe a change in one of the market data *ceteris paribus* (p. 348). So the possibilities of (exact) prediction would be rather limited.[1] But even then the equilibrium construct would still be needed for the purpose of theorizing. In fact Mises, contrary to the authors (p. 151), differentiates between different equilibrium concepts (Mises 1998, pp. 245–47).

The use of an imaginary construction of the evenly rotating economy as a counterfactual and as a conceptual foil by itself does not yet warrant any claim to the effect that it enables us to predict what is likely to happen in the real world if entrepreneurs attempt to carry out any given set of plans, in particular, as

[1]Mises recognized that "prediction can never imply anything regarding quantitative matters" (1998, p. 118). At best the use of the equilibrium construct would indeed allow us to predict merely directions or tendencies of change since, as Mises points out, "Experience of economic history is always experience of complex phenomena" (p. 348).

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will always be the case, when plans are not compatible. At least such extensive use of the equilibrium concept would require some assumption or hypothesis about how agents learn from experience, of how disappointment leads to the discovery of new information, and to the alteration of plans as transactors try to attain the most advantageous use of resources available (pp. 160–61). Even Lachmann admits that "the divergence of expectations, apart from being an obstacle to equilibrium, has an important positive function in a market economy. *It is an anticipatory device*" (Lachmann 1976, p. 131).

Much in the spirit of Lachmann's work, it is today taken for granted that the Hayekian insight into the dynamic adjustments of the process, the "extension of the equilibrium concept . . . from equilibrium analysis to the explanation in terms of causal sequences" (Lachmann 1976, p. 161; see also Hayek 1941, p. 23) constitutes a definite advance over Mises's system. Hayek himself had long maintained that his intention in 1937 had been to show Mises the deficiencies in the praxeological approach. The approach presented in the book under review gives us reason, however, to pause and to ask ourselves again whether this view is not exaggerated. From all reports Mises applauded Hayek's (1937) "Economics and Knowledge" paper and did not see it as an attack on his system while Hayek thought it was. It can reasonably be argued that the "pure logic of choice" has a great deal to say about the prerequisites for successful action—notwithstanding our ignorance as to the mechanisms of social causation (Selgin 1990, p. 29). The book under review succeeds reasonably well in consistently integrating the views of different Austrian writers.[2]

There are also differences between Mises's and Hayek's respective accounts of the business cycle. Mises's account rests almost solely upon his theory of the components of the market rate of interest (Mises 1998, pp. 535–83). The social time preference component (originary interest), the entrepreneurial component and the price premium component of interest are indispensable in Mises's account of the trade cycle. In Mises's presentation of trade cycle theory in *Human Action* the Ricardo-effect argument plays no role at all.[3] The Ricardo-effect argument is usually invoked to explain why inflation-induced shifts in resource allocation eventually will be reversed.

[2]In fact there is reason to believe that Hayek misunderstood Mises's position. This misunderstanding may be related to the positivistic idea that the propositions of logic—*in casu* the logic of choice—cannot be but "merely" analytic, that is, devoid of empirical content. Mises, however, rejected, the analytic-synthetic dichotomy (Mises 1976, p. 44). *Contra* Hayek, it can be argued that the "pure logic of choice"—better is: logic of action—is not merely a collection of empirically empty tautologies (see also Selgin 1990, pp. 28–29 and passim).

[3]Mises discusses the Ricardo effect (pp. 767–70) in *Human Action*, but in a different context.

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But again the differences between the theories of Hayek and Mises may be more apparent than substantial in this respect. Hayek invoked the Ricardo effect for a very specific purpose. Hayek provided the Ricardo-effect argument only to show how the system would act if the rate of interest failed to act at all (p. 141).[4] It is more likely that the rate of interest will play a significant role that affects entrepreneurial decisions in a manner similar to the Ricardo effect. Investment cycles typically end in a credit crunch (see also O'Driscoll and Rizzo 1985, p. 210).

Other valuable features of the book are the following:

(1) A comparison of the respective views of Hayek and Keynes that highlights not only obvious differences but also the characteristics their views have in common. Both Hayek and Keynes presented major challenges to classical monetary theory. Both felt that a monetary economy differed from a barter economy. These important differences were related to the use of money and the role of time in the economic process (p. 175). Both used the Wicksellian saving–investment approach as a basic tool of analysis and as the basis of their investigation. However, as Hicks stated, "Wicksell plus Keynes said one thing, Wicksell plus Hayek quite another" (p. 29). Whereas Keynes, starting from a model similar to Wicksell's, looked for the cause of economic problems by disaggregating the financial sector, Hayek and Mises took the Wicksellian change in the money rate relative to the natural rate as the cause of the cycle and turned toward the real sector to analyze all the effects from this monetary cause. But as is well-known, Keynes's work led to policy conclusions that were completely opposed to Hayek's policy recommendations.

(2) A lucid discussion of the three general forms of quantity equations that have appeared in the literature and of the ways in which they relate to the

[4]The authors (p. 66, n. 49 and p. 88, n. 65) mention the work of an early critic of Austrian business cycle theory, namely Kaldor (1942) who had concluded that "On second thought the theory was by no means so intellectually satisfying as it appeared at first" (p. 359). According to Cochran and Glahe (p. 88) these problems are related to the fact that:

> Hayek either abandoned or failed to fully use the monetary and capital theory framework of Mises [1971]. He used a Walrasian intertemporal equilibrium instead of the final state of rest [Mises 1966] or the Evenly Revolving Economy, . . . the Austrian equilibrium concept, and abandoned imputation theory and time preference interest theory.

Kaldor's critique is not specifically addressed, however. Criticisms along the lines indicated by Kaldor have been reiterated recently by Tyler Cowen (1997, p. 108). Cowen's comments still await detailed refutation. Cowen rejects the Ricardo effect as an account of cyclical dynamics (p. 107). Cowen points out that workers in capital-intensive industries may find that their wages rise more quickly than do the prices of consumption goods. To that extent, Cowen argues, the Ricardo effect does not reverse the capital-intensive investments (p. 107).

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Hayek–Keynes debate. Keynes's dissatisfactions with the quantity equation tools and the fundamental equations of the *Treatise* are equally briefly discussed (pp. 28–29, 31–34, 41–48). The three general forms are: (a) a transactions equation, usually attributed to Irving Fisher, and called the equation of exchange. In its simplest form, it is written MV = PT. (b) The income form of the equation of exchange, written MVy = Py. This form became popular as interest shifted from long-run to short-run problems and economists, instead of concentrating on gross transactions, began to stress income transactions (p. 36). (c) A third type of quantity equation is the Cambridge or cash balance equation, M = kPy. A simple transformation of the Cambridge equation produces an equation that looks similar to the income form of the transactions equation, M(1/k) = Py. From this transformation, it appears that the Cambridge k is the inverse of the income velocity (p. 39).

In a Hayekian structure of production framework the summation of the money expenditure streams associated with each stage would represent the PT = MV of a transaction equation (but only transactions associated with the transfer of real goods and services are included). When national income or national product is used as a foundation for macroeconomic analysis, emphasis is placed on the total money flow of expenditure on income (final) goods and the related real flows (pp. 114–15). The predominate quantity equation became the income form (p. 116).

(3) The book contains an updating and revision of the drama. The authors argue that the Hayekian model can be introduced into the current debate as a legitimate alternative not only to the Keynesian view but also to monetarist and new classical approaches (p. 12).

The first substantial achievement of the new classical macroeconomics was to incorporate the rational-expectations hypothesis into the expectations-augmented Phillips curve.[5] The Phillips curve represents an inverse relationship between inflation and unemployment. Friedman had argued that output and unemployment would deviate from their natural rates only to the degree that workers mistook inflation for changing relative prices (Friedman 1987). There is no stable relationship between inflation and unemployment, only one between *unanticipated inflation* and unemployment. At the beginning of an inflationary period, workers are slower than firms to recognize the full extent of the

[5]The rational-expectations hypothesis, in fact, is a subsidiary doctrine. The "new" element in the new classical macroeconomics, which distinguishes it from monetarism and other related schools of neoclassical macroeconomics, is the insistence that only a disaggregative, Walrasian approach will do (Hoover 1998, p. 334). But any discussion of the rational-expectations hypothesis in macroeconomics must begin with the Phelps–Friedman "natural-rate" revolution.

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inflation, so they think that the higher nominal wages being offered are actually higher real wages. With this apparent higher real wage, workers offer more labor services, people accept jobs rather than keep searching for better ones, and the unemployment rate falls. When workers realize they have misjudged the inflation rate, they withdraw some of their labor, some workers quit their jobs, and the unemployment rate returns to its "natural rate."

Rational expectations theorists began with the natural-rate hypothesis but posed the following question: Why should participants in the economy be systematically "fooled" by the inflation rate? Robert Lucas argued that the rational expectations hypothesis and flexible prices implied that such mistakes were unsystematic and fleeting.

The mistakes in predicting inflation would have to be random; mistakes can be made, but the public can never be systematically fooled about the inflation rate. Movements in the unemployment rate from its natural rate can still occur when mistakes are made in predicting inflation, but these errors must be random.

The supposedly unique or new aspect of the natural unemployment rate hypothesis is its prediction about policy. The new classical analysis of the Phillips curve led immediately to Thomas Sargent and Neil Wallace's (1975) well-known "policy-ineffectiveness proposition": aggregate monetary policy could not *systematically* alter real outcomes.

The implication of this view for the potency of monetary policy is indeed startling. The natural unemployment rate hypothesis and its later developments in the rational expectations hypothesis purported to show that any fully anticipated monetary policy would be ineffective. Only the *unanticipated* growth of the money supply can make the actual inflation rate diverge from the expected inflation rate. The theory implies that the effects of a nonaccelerating monetary disturbance will be self-reversing. An expansion of the money supply at a constant rate will cause the economy to first expand and then without any further exogenous changes contract, and vice versa for a monetary contraction. The natural-rate theory explains a cycle in terms of a response to a single "shock." In the long run, the policy is ineffective; real phenomena ultimately dominate purely monetary influences. The natural unemployment rate hypothesis has a self-reversing component.

The relevant policy elements contained in the Hayek–Mises model are similar to those contained in the natural unemployment-rate model. The need to expand the money supply at an accelerating rate to maintain the initial output effects of an expansionary monetary policy had been part of the Austrian business cycle theory long before the development of the natural-rate model. In fact, the Austrian business cycle theory like (and prior to) the natural

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unemployment theory has both a self-reversing and an accelerationist component. Monetary changes are self-reversing. A monetary expansion at a constant rate will cause first an expansion then a contraction of the economy. Monetary policy is ineffective in the long run. The increased employment initially caused by a monetary expansion can only be maintained if the money supply continues to increase at a progressive rate.

It is recognized that Hayek's monetary policy conclusions are quite similar to those arrived at on the basis of the new classical analysis. The Hayekian model implies policy ineffectiveness. Policy will not only be ineffective in the long run, it will also cause future instability. As I pointed out the so-called "policy ineffectiveness proposition" is also a celebrated conclusion of the new classical analysis (Sargent and Wallace 1975). Perhaps for this reason non-Austrian economists are largely unaware of the major differences between the Hayek–Mises theory of business cycles and the new classical equilibrium business cycle theory.

The perception of the problem that is the starting point of the analysis in the Austrian theory differs from the starting point of the new classical analysis. In the Hayek–Mises model monetary changes are the necessary cause of systematic distortions of relative prices (p. 158). As he Austrian theory is indeed a theory of "business cycles without rational expectations" (Cowen 1997, p. 76).

In fact Austrian theory assumes that market participants can be expected to make use of information conveyed by prices along with other particular knowledge that they might have, but they cannot be expected to know the parameters of the economy's structure (Bellante and Garrison 1988, pp. 213–14). Entrepreneurs will trade largely on the basis of their market savvy, which is derived from their reading of prices, wage rates, and interest rates. That is, as Hayek would say, they act on the basis of their knowledge of particular circumstances of time and place, but they cannot be expected to behave as if they have an economist's understanding of the structure of the economy. The absence of such theoretical knowledge at the level of entrepreneurs is what gives plausibility to the Austrian theory of the business cycle. For entrepreneurs to distinguish clearly and quickly between real and nominal changes would require, among other things, that they have a substantial grasp on Hayek's second kind of knowledge.

This assumption is part of the antecedent conditioning the applicability of the theory, but it is clearly at odds with the rational expectations hypothesis. The rational expectations hypothesis asserts that economic agents form expectations consistent with the forecasts of the maintained model (Hoover 1998, p. 334). It was originally formulated by John Muth in 1960, and Robert Lucas and Thomas

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Sargent introduced it into macroeconomics about 1970 as a way of avoiding the implication of the then prevailing models that economic agents made systematic and correctable errors in forming expectations.

The technical role of rational expectations is to maintain consistency: the new classicals argue that the models should not ascribe to the agents they model expectations that are different from the ones they would form as outsiders using the model to describe the economy. In other words, the model should not confer an informational advantage on the econometrician over the modelled agent (Hoover 1998, p. 336). Moreover, such expectations must be unbiased: they must not produce systematic, remediable expectional errors.

In contrast two assumptions about expectations characterize the Austrian theory: (a) the entrepreneurs do not already know—and cannot behave as if they already know—the underlying economic realities whose changing characteristics are conveyed by changes in prices, wages, and interest rates, and (b) prices, wages, and interest rates tend to facilitate the coordination of economic decisions and to keep those decisions in line with the underlying economic realities (Garrison 2000, chap. 2).

Whereas the new classical analysis the rational expectation hypothesis is conceived as a consistency axiom, that is, it refers only to the relationship between the assumption about expectations and the theory in which it is incorporated, inserting that same hypothesis into Austrian theory would involve an inconsistency. It is not logically consistent to claim (a) that the entrepreneurs already have—or behave as if they already have—the information about the underlying economic realities independent of current prices, wage rates, and interest rates and (b) that it is prices, wage rates, and interest rates that convey this information. This difference between the two models is fundamental.

Furthermore, the central feature of the Hayek-Mises discussion is the Wicksell mechanism, not the Phillips curve. In the Hayek–Mises model the real responses to a monetary change depend on the initial distribution effects of the monetary change. In the natural rate and new classical models any real responses are dependent upon unanticipated monetary changes.

According to certain radical exponents of the new classical economics, any predictable part of the money supply should have no effect on output, employment, or any other real variables in the economy. Only unpredictable money-supply changes can affect output (Sheffrin 1996, p. 35). Surely Hayek did not reach the same conclusion.

Hayek would not agree that a fully announced set of policy changes will have no effect on real macroeconomic variables. Stimulative policies may have

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short-run beneficial effects, but these beneficial effects will be at the expense of long-run disruptions (see also Moss and Vaughn 1986, p. 565).

Hayek argues that the transmission mechanism is extremely important. The path of the economy following a monetary disturbance is a disequilibrium path, not an equilibrium path. Whereas the new classical writers develop equilibrium models of the business cycle, the Hayek–Mises model provides a disequilibrium explanation of the cycle. In the Austrian model, monetary disturbances cause cycles because ordinarily reliable prices give false signals. Production is misdirected: monetary changes alter relative prices in such a way that plans based on these false prices direct the use of resources so that economic activity is not coordinated (p. 163).

Whereas the macro-adjustment processes developed in the natural rate and the new classical models are still in the form of causal relationships between broad aggregates, Hayek's predictions depend on the monetary change affecting different sectors unequally. Austrians work at a lower level of aggregation in order to allow for the outputs of the investment-goods sector and the consumer-goods sector to move relative to one another and even to allow for differential movements within the investment-goods sector. The economy's production process is disaggregated into a number of temporally sequenced stages of production. Replacing the single investment aggregate with temporally sequenced stages that make up the economy's capital structure is exactly what provides a basis for a substantive distinction between sustainable and unsustainable growth (Garrison 1996b).

The Hayek–Mises theory is capital-based, not labor-based. While monetarism and new classicism are based on the intertemporal substitutability within the market for labor, Austrianism is based on the intertemporal complementarity within the market for capital goods (Bellante and Garrison 1988, p. 208).[6]

In several ways the Hayek–Mises theory is a theory about the upward turning point and its inevitability, the end of the boom and the beginning of the downturn (p. 107). It does not rely on quits to explain increased unemployment. Instead it gives an argument that shows why excess capacity and layoffs

[6]As a reminder: in Hayek's opinion, the rate of profit is the ultimate determinant of the form of investment. In long-run equilibrium, the rate of profit is equivalent to what in the real analysis was called the rate of interest, the margins between prices and costs. It is these margins which Hayek refers to as the key relative prices that are altered by monetary changes. It is the marginal rate of profit that will adjust to the market rate of interest. However, the initial effect on price margins will be at least partially reversed. The rate of profit should first decrease and then increase. The pattern of expenditure based on the tastes of consumers will tend to reassert itself as the money expenditure flows through the system. So Austrians tell their business-cycle stories in terms of changes in the interest rate rather than changes in the wage rate, because their theory is capital based rather than labor based.

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may become prevalent (p. 195). In addition, the analysis does not rule out Keynesian type contractions following the initial crises. Once unemployed resources develop, a "secondary deflation" (Hayek 1969, p. 176) or Keynesian type collapse is likely (p. 144).

So what is the upshot? The essential insights characterizing the Austrian theory of industrial fluctuations relate to (a) the self-reversing nature of monetary changes (pp. 125–27) and (b) the accelerationist component of the theory (p. 142). The authors elucidate these characteristics with the aid of some highly illuminating numerical examples (see pp. 125–27 and 142). The Austrian business cycle theory like (and prior to) the natural unemployment theory has both a self-reversing and an accelerationist component:

(1) Relative price changes brought about by monetary factors are not equilibrium relative prices, that is, the prices are not consistent with the underlying real factors. The systematic distortion of relative prices misdirects production temporarily. But as economic agents discover that plans are not coordinated, real forces will reassert themselves. The misdirection is corrected and cyclical phenomena are observed (p. 159). This is not meant to imply, however, that relative prices will, in general, return to their previous, pre-boom levels.

(2) An injection of additional money and credit through the banking system can postpone the need for adjustment (if the stress is laid upon the word *postpone*). However, the additional increases in the effective quantity of money must be progressively larger if the new conditions are to be maintained and the crisis is to be avoided (pp. 139–40).

The policy recommendation is similar but not totally identical, however. In *Prices and Production* Hayek (1935) recommended the constant-MV norm for the exercise of monetary policy. Hayek argued that macroeconomic coordination is best promoted by a constant MV. The implied monetary rule, then, is: Increase M to offset decreases in V, but allow decreases in P to accommodate increases in T. He had not argued, however, that the rule would ensure neutrality, a view wrongly imputed to him by Sraffa (1995, p. 199; see also Steele 1996, p. 132; and Hayek 1935, p. 131).

In the monetarist view, to the contrary, so long as the price level is stable, monetary expansion is not disruptive. Monetary expansion may even be necessary to keep prices from falling during periods of real economic growth. The monetarist recommendation, then, becomes: increase the supply of money to match long-term, secular increases in real output. The Austrian recommendation, in contrast, is to abstain from monetary expansion even in periods of economic growth; increasing output should be accommodated by a declining price level (Bellante and Garrison 1988, p. 227).

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UNSOLVED PROBLEM?

The authors recognize as an important aim of business-cycle research that the understanding it provides "may help reform financial markets and banking in ways that could prevent or reduce the impact of monetary induced business cycles" (p. xi) and they state that, "It is only through a greater understanding of the forces actually shaping events in a monetary production economy that we can make rational decisions about policy and monetary institutions" (p. 166).

It is recognized that the work of Mises and Hayek can be characterized as a constitutional approach to monetary problems: the search is not for a proper policy but for an appropriate monetary framework (p. 166, n. 136). Constitutional economics examines the choice of constraints as opposed to the choice within constraints (Van den Hauwe 1999, p. 101). In recent times it was mainly through Hayek's *The Denationalization of Money* that the debate over monetary policy was again reconceived as a more fundamental debate over monetary regimes (White 1999a, p. 117).

The cycle depends on the elasticity of bank credit; the characteristic of a modern financial system that allows the supply of money credit to differ from the supply of credit based on real saving (p. 120). The operations of a developed banking financial system will tend to retard the operation of the interest-rate brake (p. 166). Economists study different institutional arrangements to determine which type of institution is most likely to minimize this tendency for the market rate to be reduced below the natural rate.

But this crucial point relating to the institutional underpinnings of the business-cycle phenomenon is not elaborated by the authors. What are the necessary or sufficient institutional conditions for the prevention of recurring business cycles and for securing macroeconomic coordination? Can economic coordination be achieved through central banking? Would it require the abolition of fractional-reserve banking? Or can economic coordination be best guaranteed by some variant of fractional-reserve free banking? It is precisely with regard to these questions and their answers that not only Hayek, Mises, and Rothbard but also contemporary economists all working within the Austrian tradition hold different and sometimes contradictory views.

Mises argued that, "Free banking is the only method available for the prevention of the dangers inherent in credit expansion" (1998, p. 440). According to Mises, "[t]he establishment of free banking was never seriously considered precisely because it would have been too efficient in restricting credit expansion" (p. 438). According to Mises natural forces limit the increase in fiduciary media.

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> A bank can never issue more money-substitutes than its clients can keep in their cash holdings. The individual client can never keep a larger portion of his total cash holding in money-substitutes than that corresponding to the proportion of his turnover with other clients of his bank to his total turnover. (Mises 1998, p. 435)

In recent times Mises's theme has been taken up again by the fractional-reserve free bankers. According to the free bankers the aforementioned constant *MV*-norm would get implemented automatically by competitive forces in a system of fractional-reserve free banking. Free banking would thus automatically discriminate between real disturbances and monetary disturbances, reacting only to the latter (Selgin 1988, pp. 64–69; see also Garrison 1996a). Given the authors' explicit acknowledgment of the influence of Roger Garrison's work on their own (p. xiii) one might guess that they would favor the fractional-reserve free banking solution but the book contains no explicit confirmation or statement of this view.

Surely there remains a certain ambiguity in this context. As the authors point out, "Hayek was arguing that cyclical activity will be a standard feature of an economy with an elastic currency; *an economy where the supply of money either wholly or partially responds to changes in the demand for money or the demand for credit*" (p. 75; emphasis is added). Although the primary concern of the fractional-reserve free bankers does not relate to the question of how to avoid Hayekian cycles, they seem to hold the view that in a free banking system where the supply of bank money responds automatically to changes in demand to hold bank money "disequilibrium situations depicted by monetary business cycle theories, such as Hayek's (1935), are avoided" (see, e.g., Selgin 1996, pp. 103–07). Only issuance of additional fiduciary media *in excess of the demand to hold them* generates injection effects and sets in motion the trade cycle.[7] However, it should be remembered that Mises held the view that "issuance of additional fiduciary media, no matter what its quantity may be, always sets in motion those changes in the price structure the description of which is the task of the theory of the trade cycle" (Mises 1998, p. 439, n. 17).

In a more recent publication the ambiguity is resolved, however. Like the Rothbardians (see Hoppe, Hülsmann, and Block 1998) the authors view money creation in a fractional-reserve banking system as a credit creation process and recognize that credit creation is not financial intermediation. Money and credit-creation is a self-reversing process with the potential to generate cycles that could be prevented by avoiding credit creation. Money is a present good. A bank deposit (redeemable at par on demand) is not a debt

[7] Larry White, personal correspondence.

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transaction but a bailment in its economic impact even if it is treated as a debt by the legal system. There subsists an important difference between Mises's approach and that of the Rothbardians, however. Mises saw no fraudulent behavior in the development of fractional-reserve banking. According to Mises "What is needed to prevent any further credit expansion is to place the banking business under the general rules of commercial and civil laws compelling every individual and firm to fulfill all obligations in full compliance with the terms of the contract" (Mises 1998, p. 440). The Rothbardians to the contrary ground their rejection of fiduciary media in libertarian ethics.

It remains an open question whether the fact that the book does not explicitly pronounce in favor of one of these solutions over another must be considered one of its virtues rather than a major defect. At this moment the debate seems to be settled in favor of the Rothbardian view that "the root of all evil" is fractional-reserve banking and fiduciary media. On the other hand it must be recognized that Hayek looked at these matters rather differently. At the time he engaged in business-cycle research, he never endorsed laissez-faire in banking. Hayek viewed the freedom of bankers to vary the stock of money as a source of disequilibrating shocks. According to Hayek the impulse initiating unsustainable cyclical booms was often the failure of the market rate of interest to rise with the equilibrium or natural rate when the demand for loanable funds increased. He elaborated Thomas Joplin's argument as to how commercial banks responded to an increase in loan demand by varying only the quantity of loans and not the price (Hayek 1935, pp. 15–17).

White has argued that the Joplin–Hayek argument is unfounded: in a competitive banking system the loan and deposit rates of interest should be expected to track the natural rate. Hayek underestimated the strength of self-correcting forces in a competitive banking system (White 1999b, p. 767).

FURTHER RESEARCH

The purpose of the authors is not explicitly polemical. The classic Mises–Hayek theory has been under serious attack recently, especially by Tyler Cowen (Cowen 1997). Except for Sechrest's (1998) critical review in this journal Cowen's "new" Austrian perspective has gone largely uncriticized until the present. It is to be expected that the book under review will serve as a useful guide to further debate and criticism in this context.

Much of Cowen's argumentation centers on the question of how likely and to what extent entrepreneurs are "fooled" or "confused" by a policy-induced change in the interest rate. The discussion takes the form of a comparison of Austrian views with those of the modern new classical school.

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Though the terminology, especially the idea of being "fooled," is common to both schools, it has a literal meaning in the context of new classical models, where the model economy is an exceedingly simple one, and a metaphorical meaning in the context of Austrian theory, which deals with a complex market system. In new classical theory, there is typically one signal—the wage rate—whose changes might be interpreted in two ways (nominal or real) and one real variable—leisure preferences—that may or may not have changed. In such a simple model economy, the theorist must contrive reasons to account for anyone ever being "fooled" by anything.[8] The standard argument involves islands whose wage rates are affected locally by real shocks and globally by nominal shocks. On a given island, there may be some confusion for a time about whether the latest shock was a real one. Hence, people may be fooled, if only temporarily, by a nominal shock. According to Cowen: "The Austrian claim postulates systematic entrepreneurial errors in the most costly possible direction. . . . The Austrian claim not only violates rational expectations but requires an especially severe *naïveté*" (Cowen 1997, pp. 81–82).

It is important to see that the theory has a conditional, that is, an *if–then* structure. Even if empirical evidence would establish, in a particular historical context, that entrepreneurs are unlikely to be "fooled" in the aforementioned sense, this kind of critique might well leave the Austrian theoretical edifice intact. The criticism would relate to questions of applicability in a particular instance, not to the theoretical validity of the theory. The theory only claims that, *ceteris paribus*, given the instantiation of the facts enunciated in the assumptions, the consequences follow. And that theory still stands.

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[8] I thank Professor Roger Garrison for having drawn my attention to this point.

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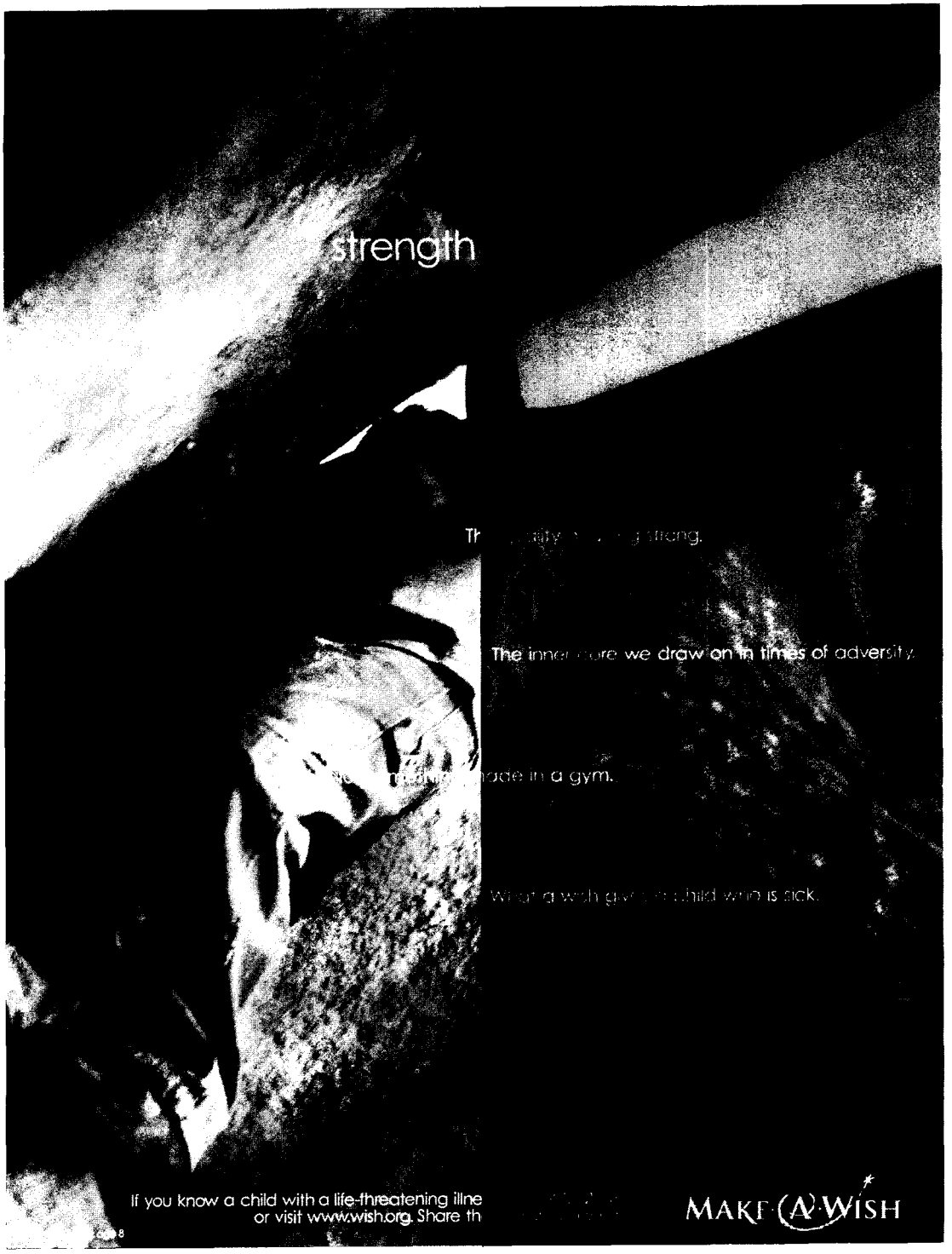

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THE MARKET: ETHICS, KNOWLEDGE, AND POLITICS. BY JOHN O'NEILL. ROUTLEDGE, 1998.

Professor O'Neill raises a large number of issues in this important book, including the value and nature of autonomy, the merits of equality, and whether political institutions should be neutral between differing conceptions of the good. (He thinks they should not be.) Although our author sympathizes with socialism, he greatly respects the work of F.A. Hayek, and he is particularly concerned to examine Hayek's epistemological arguments for the market.

I propose to confine the present examination of Professor O'Neill's book to one central topic, likely to be one of interest to readers of the *Quarterly Journal*. In these pages, and in our predecessor journal the *Review of Austrian Economics*, there has been much concern with the nature of the socialist calculation debate. Is there one calculation argument or two—one based on calculation and the other on knowledge? Did Mises and Hayek advance the same argument, with minor variations, or were their contentions quite different? Professors Herbener, Hoppe, and Salerno, on the one hand, and Yeager, on the other, have not been in full accord on these matters.

Professor O'Neill has an interesting and original view of the calculation argument, and, as will be seen, he to a large extent supports the "separationists," though with a very different assessment.

Like Herbener, Hoppe, and Salerno, our author sees the reduction of economic values to measurable units as central to Mises's case.

> For Mises any rational decision, beyond the most simple, requires the commensurability of different values. There needs to be a single common unit which reduces the choice between different options to a matter of calculation. Mises assumes an algorithmic conception of practical reason. Rational decisionmaking requires the application of mechanical procedures of calculation to arrive at a determinate answer to any question. (p. 115)

Our author has, as it seems to me, seriously mistaken Mises's views. Mises does indeed stress the need for reduction to a common unit when making decisions. But what he is concerned with are decisions that involve the use of means of production to achieve competing ends. Should I, e.g., use a supply of

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steel to produce dentures in Russian style or to service surgeons who require plates to insert in their patients' skulls?

Mises does *not* say that all decisions can be reduced to a common denominator of money prices. In particular, he does not claim that a person's ends can be reduced to a common unit of measure. I should have thought it well known that Mises believes utility is ordinal and not cardinal. So far as ends are concerned, he denies precisely the thesis that our author attributes to him.

Our author's misunderstanding leads him seriously to underestimate the power of Mises's position. Thus, he correctly points out, "Mises recognizes that, even in a market economy, there exist 'noneconomic goods,' those 'which are not the subject of exchange value.' Environmental goods provide the exemplar of these" (p. 117). In response, Mises says that those who wish noneconomic goods can, in a system of market prices, know how much these goods cost them.

About this answer, Professor O'Neill observes,

> Mises is assuming that every choice is implicitly an exercise in economic evaluation. In such hard choices, whether or not we like to admit it to ourselves, we are implicit accountants, putting a price on unpriced goods. The agent in a choice of this kind knows not only the value of everything, but also its price. (p. 117)

No, no, no! Someone who chooses a noneconomic good over a good with a price has not put a price on the good he chooses. Rather, he prefers the noneconomic good to the good of a given price. I cannot think that our author has fully grasped the distinction between ordinal and cardinal utility.

He is entirely right, though, to stress in Mises's calculation argument the importance of a reduction of production alternatives to a common set of money prices. Now begins our author's new perspective on the argument. Mises's most famous opponent, Oskar Lange, agrees with Mises about the importance of a common unit of measure.

> Lange shares Mises's positions on commensurability and the nature of practical reason. . . . The central thrust of Lange's argument is to show that there is a technical solution to the problem of choice possible within a form of socialism which has a market in consumption goods but not in production goods. (p. 119)

In an important sense, then, Mises and Lange were on the same side in the debate: both accepted what Professor O'Neill terms the "algorithmic" conception of rationality.

Opposed to them stands our author's paladin, Otto Neurath. Neurath denied Mises's fundamental assumption that choice between values requires

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commensurability. Quite the contrary, economic choices can be made by on-the-spot practical judgments that do not require reduction to a common unit. True enough, we must sometimes use general principles and rules-of-thumb in making economic decisions; but these do not support Mises's all-embracing demand for calculability.

In taking this view, our author thinks, Neurath adopted a position similar to the line of thought taken by Hayek, in his contribution to the calculation debate.

> In Hayek's work the issue of calculation is largely absent. His main contribution to the debate is to attempt to show how, given a market, one can have rational decisions without a calculation procedure for different social options. In making this move Hayek's position is closer to Neurath's than it is to Mises's. (p. 120)

Thus, with only slight exaggeration, we can say that Professor O'Neill sees Neurath and Hayek as allies against Mises and Lange.

What is one to make of all this? I think that our author overestimates the plausibility of Neurath's approach and misreads Hayek. In order to resolve questions by practical judgment, there must exist some practice in which the judgment takes place. Sammy Sosa can judge how to hit a baseball, because he is part of a social practice, the game of baseball, in which such assessments can be made. But there is no social practice called "coordinating the economy without market prices." People cannot use tacit knowledge to make decisions if there is no set of activities in which such decisions take place. Absent such a social practice of decisionmaking, Neurath has "solved" the calculation problem by stipulation.

As to Hayek, our author has fallen into error by his disregard for the context of the argument. Hayek's main contribution to the debate was his criticism of Lange, who, it will be remembered, acknowledged that Mises was correct about the need for commensurable units for calculation. It is only within the framework of this shared assumption that Hayek raises his problems about gathering information and the use of tacit knowledge. Hayek does not deny the importance of calculation: he takes it for granted.

As suggested earlier, many other topics in this book deserve our attention. Readers should not miss, e.g., the author's claim that John Stuart Mill's argument for a single measure of value commits a quantifier-shift fallacy (pp. 123 ff.).

David Gordon
Mises Review

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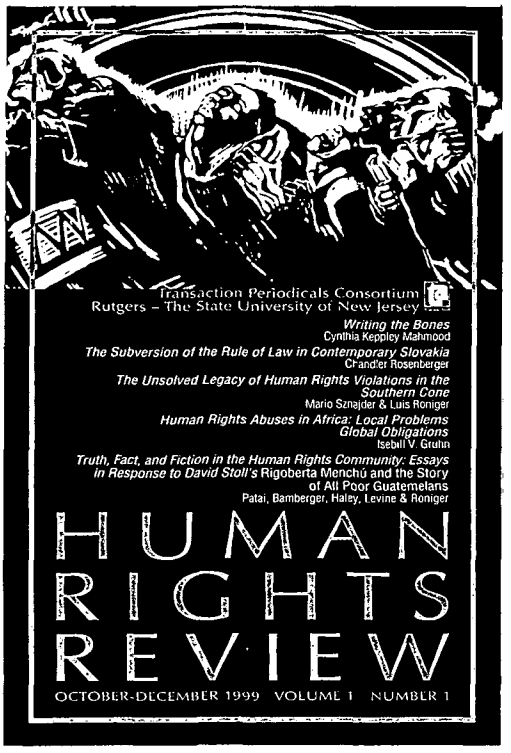
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DINERO, CRÉDITO BANCARIO Y CICLOS ECONÓMICOS. BY JESÚS HUERTA DE SOTO. MADRID: UNIÓN EDITORIAL. 1998.

Some of the most significant contributions made by Austrian economists relate to the field of money and banking. In his first great treatise, the *Theory of Money and Credit,* Ludwig von Mises improved on Carl Menger's subjective theory of value and showed how it can be integrated with the theory of money. He also developed what could be called an ontology of monetary objects, subdividing them into two main groups: (1) various types of money *per se* and (2) various types of *titles* to money like banknotes, demand deposits, etc. Mises placed particular emphasis on the analysis of the economic effects of money titles. In fact, the entire third part of his treatise is dedicated to the description of the effects brought about by the use of money titles that are *not,* or at least not entirely, covered by money in the vault of the issuing bank. Such uncovered money titles he called *Umlaufsmittel,* and as the German-language title of his work (*Theorie des Geldes und der Umlaufsmittel*) indicates, he was eager to single out the impact of uncovered money titles on the workings of a monetary economy.

Thus, Mises developed a highly original comparative analysis of what we call today fractional-reserve banking. His procedure deviates from the standard literature on money and banking in that Mises relies on a distinction that seems to relate more to legal than to economic problems. Many economists avoid this, for fear of being drawn into fruitless moral and legal debates. Indeed, is it not the chief virtue of economic science to be able to sidestep these issues? Be this as it may, the fact is that Mises's analysis *did* yield important results. In particular, he demonstrated that fractional-reserve banking was the root cause of the business cycle, arguing that the issue of uncovered money titles reduced the interest rate in the credit market below its natural level, that is, below the level it would have reached in the absence of their issue.

This perspective on problems of money and banking fell largely into oblivion. (In part that is due to the unfortunate translation of the title of Mises's work. The word *Umlaufsmittel* translates literally into "means of circulation," but was rendered in the text as "fiduciary media." To be consistent, the title of the book should thus have been *Theory of Money and Fiduciary Media,* but unfortunately this too was abandoned in favor of the stylistically smooth but toothless *Theory of Money and Credit.*) Few authors have consistently developed,

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explained, and applied Mises's approach. Most important among these are Murray Rothbard and Hans Sennholz, who in numerous books and pamphlets have stressed the economically significant differences between (market) commodity money and (government) fiat money, and between one-hundred-percent banking and fractional-reserve banking. Rothbard's great achievement was to fit the theory of one-hundred-percent as compared with fractional-reserve banking into an overall theory contrasting market actions and government interventions on both legal *and* economic grounds. Yet, neither Rothbard nor Sennholz wrote a treatise on money that is comparable to the one written by Mises, which first appeared in 1912.

The significance of Jesús Huerta de Soto's new 681-page book, *Dinero, crédito bancario y ciclos económicos* (Money, bank credit, and business cycles) is precisely that it is the first Misesian treatise on money and banking to appear since publication of Mises's original work eighty-eight years ago.

De Soto's book is both more restricted and much more ample in scope than Mises's *Theory of Money and Credit*. De Soto does not systematically deal with the principles of money, but explains them rather cursorily in his many brilliant discussions of specific issues relating to fractional-reserve banking. Thus, although his book is sound when it comes to economic principle, it is not, strictly speaking, a theory of money. Its real field is the analysis of money and banking, which it approaches from a legal, historical, and economic point of view, with a heavy emphasis on the business-cycle effects of fractional-reserve banking. Mises developed his theory of fractional-reserve banking by first analyzing the principles of money and then proceeding, step by step, to the examination of the economically relevant problems of banking. De Soto, by contrast, first examines the legal character of certain banking activities like credit banking and deposit banking, and then goes on, step by step, to point out their economic implications.

Thus, in chapter 1 he starts with a 25-page discussion of the juridical nature of deposit contracts and of the natural-law principles of deposit banking deriving therefrom. In chapter 2 there follows an extensive discussion of the violations of these principles throughout history, from antiquity over the middle ages and sixteenth-century Spain to eighteenth-century Holland (Bank of Amsterdam) and France. Chapter 3 contains a highly interesting refutation of various juridical justifications of fractional-reserve banking. Of particular interest is his critique of the most important modern justification of fractional-reserve banking, which stresses that credit contracts and deposit contracts are essentially the same thing. Huerta de Soto claims that this confusion stems from a weakness inherent in Anglo-Saxon common law, namely, that because

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of its preoccupation with individual cases, common law tends to lose sight of the larger legal issues involved in a case. It is therefore not surprising that fractional-reserve banking was legally sanctioned in Britain, whereas it was successfully countered on the Continent, where lawyers and economists trained in the Roman-law tradition approached the issue from a more systematic point of view.

Chapter 4 is the first out of five chapters containing in-depth economic analyses of fractional-reserve banking. In these chapters, de Soto's scholarship is even more impressive than in the first three chapters. He first examines the process of credit expansion (chap. 4, 78 pages) and then discusses the business-cycle effects of credit expansion (chap. 5, 102 pages). He supplements this discussion by some further considerations on business-cycle theory (chap. 6, 82 pages) and by a refutation of monetarist and Keynesian tenets on capital theory (chap. 7, 70 pages). All in all, de Soto thus dedicates 330 pages to the discussion of the business cycle and of Austrian business cycle theory. This is by far the most extensive treatment of these matters in print, setting new standards for Austrian scholars. Among the many related contributions in these chapters are his refutation of the idea that reliance on the law of great numbers can protect fractional-reserve banks from over-issues, as well as his explanation of the tendency toward centralization of the whole banking industry inherent in fractional-reserve banking.

Chapter 8 is a brilliant refutation of the rationale for both central banking and fractional-reserve free banking. Many economists past and present think that these two models are the *only* options for the organization of the banking industry. If fractional-reserve banks are inherently expansionary and unstable then it necessarily follows, according to these economists, that one needs a central bank. Yet this is a false dichotomy, which unduly eliminates 100-percent-reserve free banking from the picture and represents the advocates of fractional-reserve banking as the only defenders of genuine free banking. Huerta de Soto argues that the "false debate" between central bankers and fractional-reserve free bankers emerged during the debate in Britain between the Currency and Banking Schools. Advocates of the currency principle (in particular, John R. McCulloch and Samuel M. Longfield) adopted the above-mentioned position that a central bank was necessary to control the expansionary fractional-reserve banks. Huerta de Soto effectively criticizes central banking by showing that it is an instance of socialist regimentation, which distorts economic calculation and thus obstructs the rational allocation of resources. He applies the same criticism to fractional-reserve regimes and then proceeds to a 31-pages refutation of more recent works (by Larry White, George Selgin, Kevin Dowd, and others) written to defend this scheme.

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In his final chapter, Huerta de Soto deals with the problem of reforming our present monetary system, which combines fiat money and fractional-reserve banking under central bank control. He carefully discusses the reform proposals by Mises, Hayek, Rothbard, Maurice Allais, and the old Chicago School, and then comes up with his own proposal, favoring free choice in money and a one-hundred-percent-reserve banking system. Again, de Soto has set new scholarly standards with this detailed discussion (chap. 9, 74 pages) of monetary reform from an Austro-libertarian point of view.

In conclusion, let us observe that the step-by-step analysis in *Dinero, Crédito Bancario y Ciclos Económicos,* which starts from legal distinctions and then proceeds to discuss related economic issues, has a decidedly Rothbardian twist. Huerta de Soto's solid elaboration of his arguments along these lines makes his treatise a model illustration of the Austrian approach to the study of the relationship between law and economics. In this approach, legal distinctions are fundamental and economic analysis is a derived analysis. The latter takes up the distinctions established by the former and explores their economic significance. As de Soto's pathbreaking analysis demonstrates, this approach to law and economics is not only relevant for the understanding of micro-economic phenomena like consumer demand and pricing, but is also helpful to elucidate macro-phenomena like inflation and business cycles.

Dinero, Crédito Bancario y Ciclos Económicos is the most comprehensive analysis of fractional-reserve banking and of business cycles in print. All serious students of these subject matters will have to become acquainted with it. Let us hope, therefore, that it will soon be translated.

Jörg Guido Hülsmann
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BLACK 47 AND BEYOND: THE GREAT IRISH FAMINE IN HISTORY, ECONOMY, AND MEMORY. BY CORMAC Ó GRÁDA. PRINCETON UNIVERSITY PRESS, 1999.

The subject matter of this book is one of the great tragedies in human history. During the late 1840s more than one million Irish died and many more emigrated, with the Irish population not returning to its former level for over a century. Author Cormac Ó Gráda would appear to be well suited to write about this tragedy. He is professor of economics at the University College in Dublin and is considered Ireland's premier economic historian and a leading authority on the Great Irish Famine.

Despite his credentials and the fascinating nature of his subject matter, this book will be a big disappointment for many economists. *Black 47 and Beyond* is a work of history, not economics. It recounts the history of economic phenomena, but rarely are these phenomena subjected to economic analysis. The author shares the interests of the "new economic historians" or cliometricans who attempt to provide a statistical documentation of history. Consequently, he is less concerned with answers to the economic questions of cause and effect and often ignores the broader historical context of the phenomena he is writing about. Ó Gráda tells us how many people died, where they died, and how they died, but he does not investigate what really caused the Great Irish Famine to occur.

The author is not sympathetic to the economic theory of famines. From the ancient fathers of economics, Richard Cantillon and Adam Smith to Amartya Sen, last year's Nobel Laureate in economics, economists have generally found that famines are not natural disasters. Economists generally view famines as a result of government intervention such as war, civil war, manipulation of money and trade, or the theft of property.

Contrary to this tradition, Ó Gráda accepts the historical view that the famine was just a natural disaster. Furthermore, when he does discuss causation and blame he concludes that the English did not do enough to help their Irish neighbors. In fact, he says that the deaths and suffering should be placed on the classical-liberals and free markets for influencing the policymakers not to intervene.

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Indeed, the English classical liberals should share in the blame for the famine, but not in the manner proposed by our author. In order to understand the real causes of the famine we have to look much deeper into the historical context of the times. Ireland was a conquered land, its people were without property rights, and the English were committed to a policy of suppressing a Catholic population that continually threatened to rise up and expel their oppressors.

Prior to the famine, Ireland had been subject to invasion, war, and hegemony by the English for hundreds of years. All of the good land was taken from the Irish and given to Englishmen who were often absentee landlords. The political basis of land ownership meant that landlords held precarious property rights, which, in turn encouraged the landlords to exploit the land, rather than invest in and improve their estates.

Irish Catholics were prohibited from owning weapons and from becoming constables and were thus discouraged from accumulating capital because they could not effectively protect it. They also had no property rights or incentives to improve the land because only in Protestant-dominated areas did tenant farmers have any rights over capital improvements to the land. This may explain, in part, the Irish's over-reliance on the potato. The potato did not require capital investment, did not deplete the soil, and produced enough food to sustain families on the ever-shrinking land given to tenants.

The English had long debated the "Irish Question" prior to the famine and had concocted every imaginable charge against the integrity and nature of the Irish people. They charged that the Irish were lazy and not as industrious as the English or even the Irish who lived in Britain or America. This last observation should have led English commentators and Ó Gráda to the conclusion that Irish indolence was due not to their nature, but to English rule. Unfortunately, it did not.

Malthus led the charge that the Irish were promiscuous and married too young, which resulted in too many children, and thus a population too large for Ireland to sustain. Malthusians viewed the famine as the Irish paying for their sins. However, there is a perfectly logical explanation for Irish population carefully overlooked by Ó Gráda, Malthusians, and other English commentators. The population of Ireland grew rapidly after being incorporated into Great Britain in 1801. Populations increased throughout Europe during this period of Industrial Revolution, but the Irish population increased slightly faster than the English and European population.

This rapid population growth was caused by the protectionist Corn Laws that kept grain prices high and prevented prices from falling during years of large harvests. Originally the Corn Laws served to keep Irish grain out of

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English markets, but after incorporation in 1801, they served to keep Irish grain prices high and made Ireland the breadbasket of England. High prices brought new lands under cultivation and increased the demand for farm labor. This is when Ireland's population boomed, not before and not after. In fact, the population growth rate had already diminished substantially in the decade prior to the famine in response to reduced levels of protectionism.

The Anti-Corn Law League succeeded in 1845 in overturning the Corn Laws and establishing free trade in grain just prior to the onset of the famine. By 1847 the price of wheat had plummeted to a sixty-seven-year low. Naturally the capital value of farmland fell and the demand for labor collapsed as estates switched from grain production to pasture land for raising animals. Clearly, it was English protectionism that was responsible for Irish "promiscuity" and, in part, the massive amount of death and suffering that occurred during the famine.

Another major policy change, the Bank Act of 1844, ignited a financial crisis as the money supply shrank and credit became more restrictive. The Bank Act, no doubt, had an impact on the landlords who were often heavily in debt. While it may seem that sound money and free trade precipitated the famine, it was protectionism and government manipulation of money and banking that ultimately laid the basis of the suffering. These issues are either ignored or dismissed by Ó Gráda.

There is an implicit recognition by the author that the English are somehow responsible for Irish suffering. In fact, one of the book's strengths is the detailed information concerning the ineptitude, inefficiency, and corruption of the English-imposed Irish Poor Laws. Austrian economics, historical evidence, theory and policy analysis could have informed the author that government welfare programs are bad policy medicine, but Ó Gráda seems convinced of the mainstream view, even in the face of his own evidence, that welfare is good for poor people. In his view, English welfare policy towards the Irish needed only to be reformed, strengthened, and expanded to improve society.

The English seemed to be obsessed with policy reform and welfare measures during the nineteenth century. Real problems did exist but they had real causes that could not be addressed with reform and welfare measures. The English would have been better served if they simply had maintained peace and property rights and had stuck to a policy of sound money and no taxation.

This "reform" obsession was combined with another obsession of the English economist–philosophers to make welfare as distasteful as possible so as to discourage people from becoming dependent on public assistance. The

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English imposed a "poor law" on Ireland that called for workhouses and public works in direct opposition to its own commission that recommended investment and private charity as the remedy to poverty.

The national system of workhouses was completed just prior to the onset of the famine. Conditions in the workhouses were poor and they were often overcrowded. Food was meager and pay was very low by design. Ó Gráda even hints that they may have contributed significantly to the high death toll because deadly diseases spread quickly in the crowded workhouses.

The public works system also failed to stop the famine. People would often have to lose or forfeit all their property and possessions before becoming eligible for jobs on public works. Pay was set so low that it would not pay for enough food to make up for calories lost from the hard manual labor. Jobs were limited to one per family and the work was carried out in winter months when the poorly clothed Irish normally stayed inside to avoid the cold, wind, and moisture. As a result, the public works system further weakened the Irish people. It brought family leaders together to work under harsh conditions where they could be exposed to deadly disease, and then sent them home to expose their families to the diseases.

The public works themselves were almost entirely make-work, New Deal-style projects that provided no lasting benefit for the already capital-poor economy. To the extent that welfare was financed by local taxes, the programs further weakened the local economy. To the extent the programs were financed by England, the programs crowded out private charity from abroad. Just as is true today, people give less money to charity when they think the government is taking care of a problem with their tax dollars. Indeed, our author found that the most effective welfare measure occurred in 1847 when the English temporarily set up soup kitchens similar to those private charity had successfully provided the rural Irish poor during past crop failures. The program was quickly scrapped.

Taken as a whole (invasion, war, theft of property, disenfranchisement, barriers to entering into certain professions and bans on owning weapons, forced hard labor in camps under horrible conditions—based largely on race and religion—with millions either dying or fleeing the country) the Irish famine has much in common with the German Holocaust.

Of course, there is no convincing evidence that the English did all of this on purpose. Pro-Irish accounts of the famine are not considered academically reliable. However, Ó Gráda does provide evidence that Catholics were more likely to die than Protestants and, more pointedly, that fighting-aged men died in large numbers during this famine, while in all other famines it is usually the

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young and old that die in large numbers while the adult men are more likely to survive. It should also be remembered that these were revolutionary times and that the English were always worried about an Irish revolt. Ireland had risen up against England before and would do so again after the famine.

This is the economic background of the Irish Famine and this story is not told by the author. Many of the historical aspects of the famine are examined in significant statistical detail, but ultimately these refined empirical results paint no clearer or more certain a picture of the famine than the ballads, folklore, and newspaper accounts that the author dismisses. The important point to be remembered is that the free market did not cause the famine nor did the potato blight that struck around the world during the 1840s. Like other famines in history, the Irish genocide was the result of government at work.

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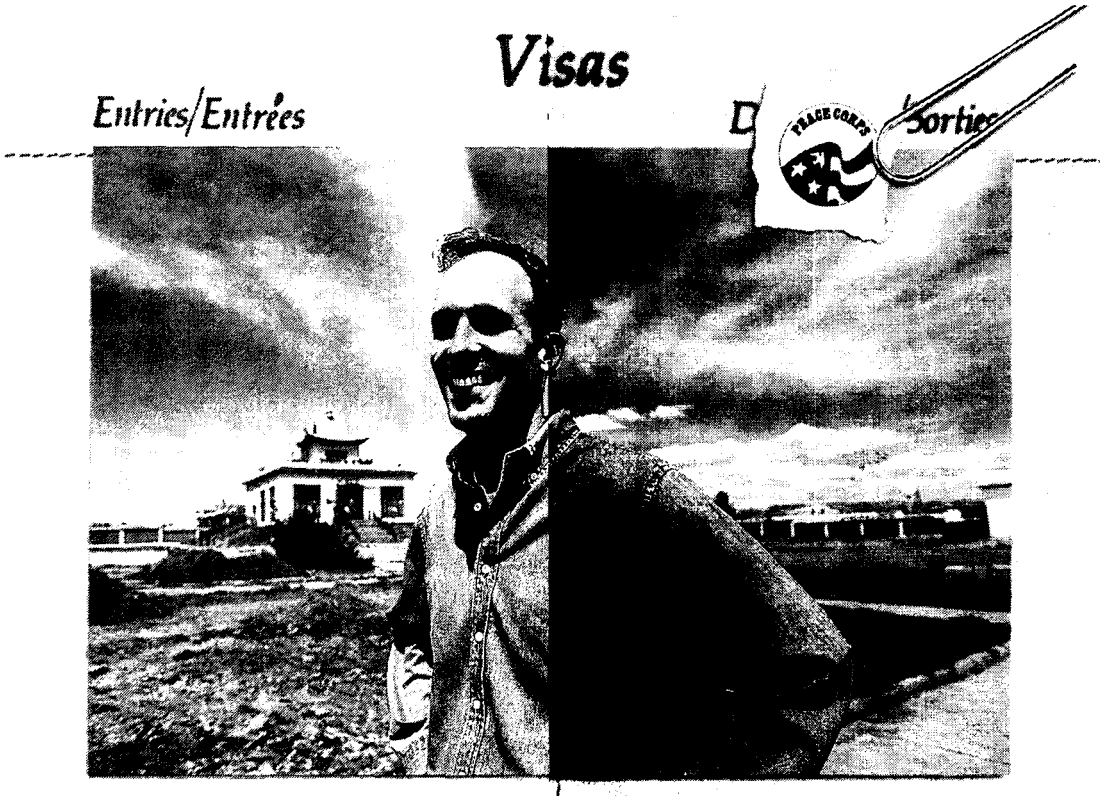

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